$7.00 per volume
Single numbers $4.00

QUARTERLY
Two volumes per year

July, 1932
Volume VII, Number 1

Founded in 1927 by Edward Bradford Titchener and Carl Murchison

THE JOURNAL OF GENERAL PSYCHOLOGY

Experimental, Theoretical, Clinical, and Historical Psychology

---

JULY, 1932

Chaque article est suivi d'un résumé en français
Jedem Artikel wird ein Referat auf deutsch folgen

LEWIN'S CONCEPT OF VECTORS 3
By Edward C. Tolman

MATING BEHAVIOR IN THE JAPANESE BEETLE, *POPILLIA JAPONICA* NEWM. 16
By Charles Marc Pomerat

A FURTHER STUDY OF GRASPING 34
By H. M. Halverson

EFFECTS OF PRACTICE UPON THE METABOLIC COST OF MENTAL WORK 65
By G. H. Rounds, H. J. P. Schubert, and A. T. Poffenberger

CONDITIONING AS A FUNCTION OF THE INTERVAL BETWEEN THE CONDITIONED AND THE ORIGINAL STIMULUS 80
By Helen Morrill Wolfle

SIMPLE CONDITIONING AS A METHOD OF STUDYING SENSORY DISCRIMINATION AMONG IDIOTS . . . 104
By Edgar A. Doll and Cecelia G. Aldrich

THE POSTURAL MODEL OF THE HEAD AND FACE IN VARIOUS POSITIONS (EXPERIMENTS ON NORMALS) 144
By Nathaniel Ross

(over)

Worcester, Massachusetts
Copyright, 1932, by Clark University
Entered as second-class matter December 1, 1927, at the post-office at Worcester, Mass., under the Act of March 3, 1879

THE EFFECTS OF COACHING SUBJECTS IN THE RECOGNITION OF FACIAL EXPRESSION 163
By Arthur Jenness

SHORT ARTICLES AND NOTES 179
A Classification for Psychology (Raymond Royce Willoughby) 179
Foveal Sensitivity as Influenced by Peripheral Stimulation (Frank A. Geldard) 185
The Measurement of Retinal Illumination (D. McL. Purdy) 189
Differences in the Recognition of Facial Expression of Emotion (Arthur Jenness) 192
Does Post-hypnotic Amnesia Apply to Practice Effects? (E. F. Patten) 196
Emotion, Intelligence, and Introversion (Edwin G. Flemming) 201
Constant Errors in the Attempted Measurement of Introversion (Joseph V. Hanna) 205
Physiological and Psychological Causes of Ocular Fatigue (J. I. Kurtz) 211

APPARATUS 214
The Behavior Research Photopolygraph (Chester W. Darrow) 214
A Method of Measuring Difference Color Limen by Means of the Photo-electric Cell with Thermionic Amplification (Christian Paul Heinlein and Lucille E. Morgan) . . 219

BOOKS 231
Edward S. Robinson, *Association Theory To-day* (John A. McGeoch) 231
R. A. Houstoun, *Vision and Colour Vision* (C. H. Graham) . 237
J. W. Dunlap and A. K. Kurtz, *Handbook of Statistical Nomographs, Tables and Formulas* (Walter S. Hunter) . . 239
Books Received 240

34-127
12-7
13

LEWIN'S CONCEPT OF VECTORS

From the Psychological Laboratory of the University of California

EDWARD CHACE TOLMAN

I. INTRODUCTION

In a recent publication (5) Lewin has for the first time made available to English readers his concept of the "environmental field" with its component "field-forces" or "vectors." The environment for any organism he conceives as a psychologically conditioned "field." The objects in this "field" are, by virtue of the organism's fundamental or secondary needs (*echte Bedürfnisse* and *Quasibedürfnisse*), loaded with positive and negative "valences" (*Aufforderungscharaktere*). Those objects which the organism is drawn towards are said to have positive valences, those from which he is repulsed are said to have negative valences. Environmental objects with valences upon them are the points of origin of field-forces of attraction and repulsion—forces which have both quantity and direction and which Lewin, therefore, conceives as "vectors."

I shall not here attempt any complete, or indeed altogether impartial, summary of this doctrine with all its ramifications and its very great suggestiveness. I wish rather to attempt a critical attack upon one phase of it, viz., upon the concept of *vectors*. But first it must be noted that I also in a recent book (7) (in an attempt to explain the behavior of rats) was led more or less independently[1] to adopt the concept of an environment field. I called it the means-end-field or, in some places, the sign-gestalt-field. (See 7, Chapters VI to XI.) And this concept of the means-end-field was certainly very close to Lewin's "environmental field."

A means-end-field I conceive as constituted by all the objects[2] which, on a given occasion, are in the organism's actual physical and social and ideational environments (insofar as he is capable of being sensitive to them), plus the "direction" and "distance" relations of these objects to one another and to the organism (insofar as he is capable of being sensitive to such direction and distance relations) plus the fact of positive and negative "demands" of varying strength for and against such objects.

---

[1] Perhaps "less" rather than "more," since at the time of writing I was already relatively familiar with some of Lewin's earlier articles, especially (6) and my own thinking was undoubtedly much (if not altogether consciously) influenced by these articles.

[2] The term "objects" I use to cover both those relatively vague entities which we ordinarily call situations as well as those more precisely delimited ones to which the designation "object" is usually more appropriately restricted.

My "means-end-field," like Lewin's "environmental field," is thus the environmental milieu (insofar as the organism is sensitive to it) at the center of which he finds himself and relative to which he behaves. This milieu radiates out from the organism as a center. And the objects along its radii are in varying degrees demanded-for and demanded-against (loaded with positive or negative valences). All such objects have, relative to one another and to the organism, all sorts of complicated interrelations of "distance" and "direction" (Lewin introduces the term *Topologie* for these). Finally, the ultimately to-be-sought or to-be-avoided objects are states of physiological quiessence and states of physiological disturbance (*vide*: Lewin's *echte Bedürfnisse*). All other objects are good or bad insofar as they constitute routes or barriers (*vide*: Lewin's *Quasibedürfnisse*) to or from such ultimate physiological goods or bads.

In short, the means-end-field, as I conceive it, is practically identical with Lewin's "environmental field." The differences would at first sight appear to be ones of terminology only. And yet a further analysis will reveal, I think, certain real, fundamental divergences. And these real divergences arise primarily, it seems to me, relative to his concept of "vectors." Lewin is something of a Gestalt Psychologist and, as such, he has a tendency not to go in too much for analysis. But it is in this very eschewing of analysis, especially as it finds expression in this concept of vectors, that I find myself differing from him.

His vectors or field-forces perform for him a manifold function. They combine into one concept what to me, it seems, are the three mutually distinguishable and relatively independent variables of "direction," "distance," and "demand-value." And then, in addition, these same vectors are made by him to serve not only as thus descriptive constitutents of a field as already achieved, but also as causal determiners of new fields, or, at any rate, of changes in the old fields.

I, on the other hand, conceive that the causal conditions constitutive of a given field are certain deeper-lying *motivational* and *cognitive* processes released by the presented stimuli and the physiological conditions. As a result of these latter the organism elaborates a *cognitively* arrived at manifold of surrounding objects as lying in such and such "distances" and "directions" relative to one another and relative to himself. And then these various objects become further endowed with positive and negative, superordinate and subordinate "demand-values" by virtue of the organism's organic (i.e., *motivational*) needs at the moment. Direction and distance relations, on the one hand, and demand-values, on the other, are thus, as I see it, mutually distinguishable and *relatively* independent variables of any given field. Their causes are *cognitive* and *motivational* processes *behind* the field and these causes are not to be confused with these resultant features of the field themselves. Further, whereas the causes of the direction and distance features are primarily cognitive, those of the demand-value features are primarily motivational. Lewin's method of combining both sets into the single concept of vectors seems to me impermissible and confusing. But let us turn to concrete illustrations.

II. Latent Learning of Rats

First, by way of introduction and before turning to some of Lewin's own examples, I would like to cite certain rat experiments which indicate, it seems to me, a first reason why such a separation of the cognitive and motivational factors is both desirable and necessary. The experiments I have in mind are those of Blodgett (1) and the followers-up of his method (2, 3, 8, 9)[3] and are concerned with what he christened "latent learning." In general, Blodgett's procedure and that of the subsequent users of the method consisted in running rats through a maze without any reward (other than being removed when they finally reach the end-box) for a considerable number of days, and then of suddenly introducing reward. Blodgett found that, whereas during the non-reward period the rats of this experimental group made little apparent improvement in the selecting of the correct paths as against the blind-alleys, once these experimental rats had experienced the reward, their error curves dropped almost immediately to that of control groups which had been rewarded from the very beginning.

---

[3] For a brief résumé of all save the last of these experiments on "latent learning," see Tolman (7, pp. 343 f.).

Now the significance of these findings is that they indicate that during the non-reward (i.e., the highly non-motivated) period the experimental rats were none the less acquiring the same information about the lay-out of the field (i.e., the relative "directions" and "distances" of the true paths and the blinds) as were the control rats who *were* being rewarded and hence strongly motivated all the time. This information did not, to be sure, appear in the behavior of the experimental rats until after motivation had been introduced. But the fact of the immediate drops in their error curves which then did appear indicated that such cognitive information of the lay-out had been acquired by them none the less. Or, in terms of the present argument, this indicates that the means-end-fields of these experimental rats, during the period in which they were not strongly motivated, were being built up, as far as the "direction" and "distance" features were concerned, practically as rapidly as were those of the control rats. The differences between the two groups were differences in the distributions of demand-values over their fields and not in the cognitively achieved direction and distance lay-outs of those fields. And, such being the situation, it seems evident that in this specific type of case Lewin's method of combining direction and distance, on the one hand, and demand-values, on the other (i.e., primarily cognitively caused and primarily motivationally caused features), into the one single concept of "vectors" would have been decidedly misleading and confusing.

But let us turn now to some of Lewin's own applications of this concept of vectors. He uses this concept most frequently and most interestingly[4] in describing certain types of behavior in children.

III. The Very Young Child

One of the first applications which he makes is in the description of the behavior of the very young child who is positively attracted towards a goal but where a spatial barrier intervenes. In order to reach the goal the child would have to go around the end of the barrier. But this the relatively young child is unable to do. Lewin explains this, apparently, by asserting that the vectoral force exerted by the positively attracting goal pulls directly towards the goal and hence *perpendicularly* to the barrier; the young child thus cannot pull himself out of the direction of the perpendicular. The vector is too strong for him. And it appears, further, that if the barrier has—so to speak—flanges running down either side, it becomes even more difficult for the child to go around for then the initial direction of the roundabout route is even more opposed to the perpendicular pull of the vector. Older children, on the other hand, *can* go around barriers. And this, he implies, is because for older children such detour problems present no difficulty "because they [such older children] already have a sufficient survey of such situations . . . For them, it no longer requires a special act of intelligence in order that, instead of the spatial directions, the *functional* directions become decisive for the movement" (5, p. 106).

---

[4] Indeed, all of Lewin's findings are so interesting and suggestive that any criticism of his concepts such as I am here attempting may well seem a mere work of supererogation.

The young child is pulled by the perpendicular vector. The older child, because of his wider information, is not so pulled. But what does this mean in terms of vectors? Lewin does not make clear. Are there new vectors set up for the older child which point in roundabout fashion (the concept of curved vectors would be a new one in mathematics)? Or do the roundabout routes really become for the older child, functionally speaking, perpendicular routes and does the spatially perpendicular route become for such an older child functionally non-perpendicular? And, even if this latter be the case, why does the vector always pull perpendicularly? Lewin is not clear on any of these points.

In terms of my concepts, however, all that I have to do is to point out that the difference between the young child and the older one is a difference in cognitive sensitiveness[5] to direction and distance relations, including the fact of "barredness." The older child is cognitively sensitive (behavioristically speaking) to the directions and distances involved in roundabout routes and to the fact that the straight route is barred in a way that the young child is not. The young child is sensitive to the direct route only and relatively insensitive to the barred character. The older child, if he find the direct route barred, will then try the indirect ones because he is cognitively sensitive to the possibility of such indirect routes in a way that the younger child is not.

---

[5] The term cognitive sensitiveness must be understood throughout as referring not only to such sensitiveness as results from the pure intelligence-level of the organism but also to such as results from the amounts and kinds of his specific past experience.

But one further point is also involved. I am also assuming, it must be noted, that, other things being equal, a direct route always gets more subordinate demand-value distributed onto it than does an indirect route. The demand-value of the goal lends subordinate demand-value or attractingness to *all* routes but, to the degree of the organism's cognitions of the relative directnesses of these routes, the goal lends more subordinate attractingness to direct routes than to indirect ones. When, however, the direct routes are barred, then these latter, by virtue of a further cognitive sensitiveness to their "barred-character," will lose some of their relative attractingness and the indirect ones will become relatively more attracting. The difference between the younger and the older child is then, according to me, simply one of differences in knowledge-possibilities as regards "direction" and "distance" and "barred-characters" and the fact that these knowledge-possibilities are greater in the older child than in the younger.

Turn to another example.

IV. The Excessively Strong Valence

A child, Lewin points out, finds the indirect route difficult, not only if he be young, but also if he be excessively motivated. "If we continue to strengthen the valence," says Lewin, "the solution of the task ceases to be facilitated and instead becomes more difficult. The strength of the attraction then makes it doubly difficult for the child to start in a direction opposed to the field force" (5, p. 105).

How is this to be explained? According to Lewin the explanation again is in terms of vectors. As the vector is increased, due to the increased valence, the organism finds it harder to go by indirect routes. But Lewin does not offer any mechanism for this greater inability to do the indirect as the pull is increased other than again to imply the solely perpendicular action of the vectors. But why a strong vector should have a relatively stronger perpendicularity than a weak vector is not made clear. And it is this which must be clarified if his argument is to be valid.

I, in contrast, would declare that the failure to take the indirect routes, as the valence (i.e., the demand-value) of the goal-object is increased, implies an interesting and important interrelation between motivation and cognition. It implies that in states of over-strong motivation an organism has a tendency to revert to simpler levels of cognition. The older child in a condition of moderate motivation is quite able to appreciate the possibility of the indirect routes and the further fact that the direct route is barred. This same older child, on the other hand, when unduly motivated, loses this, its more sophisticated cognitive level, and reverts to the cognitive level of the younger child. The older child *regresses* (at least temporarily) to a knowledge of the direct route only and to an ignoring of its barred character. In other words, it seems that something similar to the psycho-analytical concept of "regression" is here indicated. Under the stress of undue motivation there is a regression to a cognitive appreciation appropriate only to a younger age-level. The higher, more difficultly acquired levels permitting a cognitive appreciation of the indirect routes and the barred character of the direct route drop out under the stress of such a too strong motivation.

V. THE "ALMOST" SITUATION

Again, to take still another example, Lewin points out that an "almost obtained" goal also exerts a peculiarly strong influence in making indirect routes difficult. For instance, to draw an illustration from Köhler and the dog (4, p. 14) (similar findings are obtained with the young child), if the goal, food, is put beyond the barrier but very close to the latter so that it really is just under the dog's nose, then the dog finds it much more difficult to take the roundabout route than if the food be thrown farther back from the barrier. Or consider a more complicated example cited by Lewin (one involving the social dimension):

"As an example of the increase in the strength of the valence with *temporal proximity,* it may be pointed out that, among the inmates of homes for delinquent children, reform schools, and similar institutions, it is not infrequently observed that they become especially difficult just before their discharge. We noted this paradoxical behavior, so sharply opposed to their own interests especially in previously well-behaved individuals. The essential reason was found to be the following: Even for the youth who is at first well-behaved in the home the wish for freedom is an important motive in his behavior. But at first this freedom is a distant half-imaginary goal, and most important, good conduct in the home is the way that shall ultimately lead him there. Now that his discharge is approaching, the longed-for, but until now uncertain, world of freedom is just ahead. The boundary of the home thereby acquires in much greater degree the character of a marked *barrier* which separates the youth from his almost-attained goal. Hence the home acquires a pronounced negative valence" (5, pp. 108f.).

But this, as I undersatnd it, means, according to Lewin, that the almost-obtained goal pulls uniquely and with undue strength (hence the greater impossibility of the non-perpendicular route of "waiting and good behavior"). It is, in short, in so far another case of undue motivation. But it also seems to mean for him, in addition, that the almost-situation lends an independent negative valence to the intervening barrier in and of itself so that this barrier will come to be attacked directly and for its own sake. But why these should be, Lewin, again, does not make at all clear.

My interpretation would be that, when the goal is just beyond the barrier, the ratio of the indirect-pointing segments of the necessary roundabout path to the direct-pointing segments of that path is relatively much greater than when the goal is considerably farther behind the barrier.[6] But, as I have already asserted, an indirect route always has, as such, less subordinate demand-value distributed to it than a direct route. Hence, when in the almost-situation the ratio of indirect to direct of the roundabout path becomes greater the attractingness of that path will become less (save, of course, for the completely barred character of the straight path). Hence, if the individual be also somewhat regressed or over-motivated and is thereby rendered insensitive to this barred-character, then this change in ratio works and he chooses the straight path. The inmate of the institution is over-motivated and regresses easily. The roundabout path loses more and more of its relative demand-value as the goal gets nearer, and so, if he regresses, he will fail to note the barred character of the straight path and will attempt the latter. That is, he will attack the rules of the institution and try to escape directly. Again it is a case of the limits of cognition plus the effect of motivation in inducing resulting shifts in such cognition to a lower level.

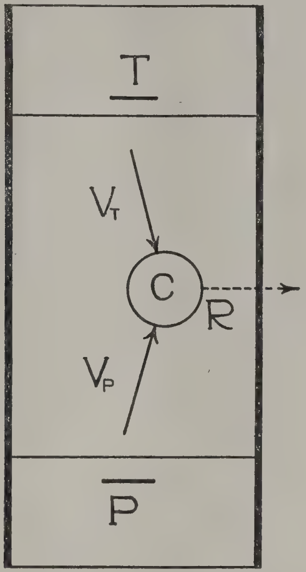
Consider one final illustration.

VI. The Conflict between Two Opposing Negative Valences

Among the most interesting of Lewin's examples are those in the realm of what he calls conflict. And as illustrative of these we may take the following: The child is placed between two negative valences—a disagreeable school task (T) and a threatened punishment (P) for not doing the task. He indicates this situation by the accompanying diagram (5, p. 111) and gives the following explanation:

---

[6] The reader will readily see the truth of this if he will draw a diagram.



A "conflict situation occurs when the child stands *between* two *negative* valences, for example, when it is sought by threat of punishment (P) to move a child to do a task (T) it does not want to do. Such a situation results in an especially strong tendency to go out of the field, in accordance with the strong sidewise resultant (R) of the two vectors (V_p and V_t). Consequently, in such a case, the child always goes out of the field unless other circumstances prevent it. Hence, if the threat of punishment is to be effective, the child must be so inclosed by a barrier (B) that escape is possible only by way of punishment or by doing the disagreeable task" (5, pp. 111f.).

We have here, it seems, a new principle—that of a resolution of forces. The two opposing vectors each give rise to a sidewise component. Two vectors thus compound into one sidewise one.

But in criticism it must be noted that it now appears *for the first time* that there are not only perpendicular components to Lewin's vectors, but also sidewise ones. Further, it is only by drawing the two opposing vectors V_t and V_p at a slight angle (which does not accord at all with his previous practice and implications) that he is able to indicate any such sidewise components. In all the previous examples vectors have been essentially perpendicular. It was because they *were* perpendicular that the organism was not able to pull away from them and go by indirect routes. But now it is admitted that vectors have some sidewise components. The doctrine we again must conclude is confused and misleading.

My explanation of this case of conflict would be simply that in the case of any negative valence an organism is always cognitively ready for any of all the possible directions radiating away from the locus of this valence. He appreciates equally readily *all* directions that point away. And in the present set-up this means that he naturally selects a final direction (i.e., the sidewise one) which simultaneously radiates away from both the negative goals, P and T. Again my explanation is in terms of the organism's degree of cognitive appreciation of direction and distance relations of the different alternative routes to or from goals and of the degrees of subordinate demand-value which therefore are distributed over these various routes.

VII. Summary and Conclusion

If we review the above cases I think we are ready now to sum them all up by saying that the concept of vectors is bad because it attempts to combine into one variable the following three: (C) the variable of the cognitively appreciated directions, distances, and barred-characters in a field; (M) the variable of a fundamental, motivationally conditioned, demand-value of the given goal or goals in this field; and (D) the variable of the final subordinate demand-values of the various alternative routes to or from such a goal or goals.

It appears further that it is the (D) which is the final variable we are interested in for it is the (D) which is the immediate cause of the behavior.

Further, it appears that under different conditions this (D) may result from different sorts of interaction of (M) and (C).

It is indeed this fact of these different sorts of interaction of (M) and (C) as the cause of the final (D) which have been illustrated by the different examples cited. Thus briefly to review these once again we may now epitomize each as follows:

1. The case of the "latent learning" of the rats is one in which the final (*D*) arises directly from a normal (*C*) [acquired without any strong (*M*) during the latent learning period], plus the later appropriate action of a newly introduced strong (*M*) in distributing demand-values over this (*C*).
2. The case of the "very young child" is one in which the final (*D*) corresponds to the action of an ordinary, average (*M*) upon an undeveloped, childlike (*C*).
3. The case of the "excessively strong valence" is one in which a very strong (*M*) causes a distortion (regression) of the (*C*) to the level of that of a much younger child so that the resultant (*D*) is thereby correspondingly primitive.
4. The case of "the almost situation" is one where actions similar to those just cited under (2) and (3) are both operative. That is, the final (*D*) in such a case results partly from a field-lay-out tending inherently to an initially weak and imperfect (*C*), and partly from a regression to an imperfect (*C*) due to the action of an over-strong (*M*).
5. The case of the "conflict between two opposing negative valences" is one where the final (*D*) is the normal expression of two negative (*M*'s) acting upon a normal (*C*).

In a word, it is just these interesting differences in the possible types of interaction of (*M*) and (*C*) in producing the final (*D*) which Lewin is interested in showing and yet any precise treatment of which his concept of vectors obscures. For this concept of vectors seems to combine (*C*), (*M*), and (*D*) into one unanalyzed and unclear hodgepodge.

Finally, however, as a last word let me emphasize that in thus attacking the concept of vectors I am merely desirous of bringing to light an issue and not of dogmatically asserting that Lewin himself does not, or could not, meet that issue. Lewin's positive contributions are so very great that any minor criticisms such as I am here suggesting may well be merely captious. And the answers to those criticisms may actually have been implied if not yet stated in what Lewin has already written.

References

1. Blodgett, H. C. The effect of the introduction of reward upon the maze performance of rats. *Univ. Calif. Publ. Psychol.*, 1929, **4**, 113-134.
2. Elliott, M. H. The effect of appropriateness of reward and of complex incentives on maze performance. *Univ. Calif. Publ. Psychol.*, 1929, **4**, 91-98.
3. Haney, G. W. The effect of familiarity on maze performance of albino rats. *Univ. Calif. Publ. Psychol.*, 1931, **4**, 319-333.
4. Köhler, W. The mentality of apes. (Trans. by E. Winter.) New York: Harcourt, Brace, 1925. Pp. viii+342.
5. Lewin, K. Environmental forces in child behavior and development. Chap. 4 in *A handbook of child psychology,* ed. by C. Murchison. Worcester, Mass.: Clark Univ. Press; London: Oxford Univ. Press, 1931. Pp. 94-127.
6. ————. Vorsatz, Wille und Bedürfnis mit Vorbemerkungen über die psychischer Kräfte und Energien und die Struktur der Seele. Berlin: Springer, 1926. Pp. 92.
7. Tolman, E. C. Purposive behavior in animals and men. New York: Century, 1932. Pp. xiv+463.
8. Tolman, E. C., & Honzik, C. H. Introduction and removal of reward and maze performance in rats. *Univ. Calif. Publ. Psychol.*, 1930, **4**, 257-275.
9. Williams, K. A. The reward value of a conditioned stimulus. *Univ. Calif. Publ. Psychol.*, 1929, **4**, 31-55.

University of California
Berkeley, California

LE CONCEPT DES VECTEURS DE LEWIN

(Résumé)

Le concept des vecteurs de Lewin (*Voir:* K. Lewin, Environmental forces in child behavior and development, *Handbook of Child Psychology,* Worcester, Mass., Clark University Press, 1931, 94-127) combine en un concept trois sous-variables qui sont réellement capables d'une variation indépendante et qu'on ne doit pas séparer par conséquent. On peut désigner ces variables comme (C) l'appréciation *cognitive* par l'organisme des relations de distance et de direction du champ environnant, (M) une valence conditionnée par les *motifs* et attachée à quelque entité dans ce champ, et (D) les valeurs résultantes *exigées* pour certaines routes au lieu d'autres comme des routes courtes pour s'approcher de cette entité du champ ou pour en partir.

Les divers exemples du comportement des enfants lesquels Lewin essaie de décrire et d'expliquer par son concept des vecteurs (et aussi le cas de l'apprentissage "latent" chez les rats) peuvent être expliqués clairement et sans ambiguité en termes des concepts donnés ci-dessus, (C), (M) et (D) et des types uniques de l'action intermédiaire de (C) et de (M).

Il paraît d'ailleurs que Lewin s'intéresse vraiment à de tels types uniques de l'action intermédiaire entre (M) et (C) et il semble à l'auteur qu'on ne peut les décrire que confusément en termes de son concept seul des vecteurs.

TOLMAN

DER LEWIN'SCHE VEKTORBEGRIFF (CONCEPT OF VECTORS)

(Referat)

Lewin's Vektorbegriff (vide K. Lewin: Environmental forces in child behavior and development, *Handbook of Child Psychology*, Worcester, Mass., Clark University Press, 1931, pp. 94-127), vereint in einem Begriff drei Untervariablen (sub-variables) die eigentlich zu selbstständiger Varirung fähig sind und die deshalb einzeln genommen werden sollten. Diese Variablen können bezeichnet werden als: (C) die Erkenntnisschätzung (cognitive appreciation) der Distanz- und Richtungsbeziehungen des Umgebungsfeldes (environmental field) durch den Organismus; (M) die durch Antrieb (motivation) bestimmte Wertigkeit (valence) die mit irgend einer Entität in diesem Felde in Verbindung steht; und (D) resultierende Anspruchswerte (demand values) die gewissen Routen anderen vorziehen als kurze Wege zur Erreichung oder Verlassung dieser oder jener Entität im Felde.

Verschiedene Beispiele, bei Kindern, von Formen der Tätigkeit die Lewin durch seinen Vektorbegriff zu beschreiben und zu erklären versucht (und auch der Fall des "latenten" Lernens bei Ratten) lassen sich klar und unzweideutig darlegen durch Benutzung der oben genannten Begriffe C, M, und D und einzigartiger Wechselwirkungen von C und M.

Es zeigt sich ferner, dass es eigentlich solche einzigartige Wechselwirkungen sind die Lewin besonders interessieren, und diese,—so scheint es wenigstens dem gegenwärtigen Verfasser—lassen sich nur verwirrt durch L's einzelnen Vektorbegriff beschreiben.

TOLMAN

MATING BEHAVIOR IN THE JAPANESE BEETLE, *POPILLIA JAPONICA* NEWM.

From the Department of Biology, Clark University

Charles Marc Pomerat

I

Insects were used in the first quantitative observations of homogamy in invertebrates. Tower (4) in 1906 studied the potato beetle, *Leptinotarsa,* but, unfortunately, as has been pointed out by Crozier (1) in 1923, his account of the matter is not at all clear. The measurement used apparently was elytron length. The data collected by Tower are given in Table 1.

TABLE 1

Assortive Mating Found in *L. oblongata,* Size Being Taken as the Selective Character

Sex	Normal range of variation—class									
	1 %	2 %	3 %	4 %	5 %	6 %	7 %	8 %	9 %	10 %
Males	0.5	1.0	3.0	6.0	15.0	40.0	17.0	10.0	6.0	1.5
Females	2.0	3.0	4.0	4.0	5.0	12.0	38.0	19.0	10.0	3.0

Percentage of Males of Each Class Which Mated with Individuals in the Various Classes into Which the Females Were Grouped with Regard to Size

Class of females	Class of males									
	1	2	3	4	5	6	7	8	9	10
1	90	10	2							
2	6	70	6							
3	4	13	71	13	1					
4		7	12	74	10	5				
5			8	12	76	10	5	1		
6			1	1	11	70	11	2	3	
7					2	13	82	85	6	1
8						2	2	10	88	3
9								2	2	6
10									1	90

Tower points out from these results that matings between males and females of very unlike size do not occur at all in the series, and that the number of unmated individuals in any population is fai greater among the extremes than among the more mediocre individuals. He concludes that selective mating exists in these beetles, though only in the character of size, and that this is due to the inability of abmodal individuals to perform the sexual act.

By dividing the percentage of each "class of males" by the percentage of their respective "class of normal range variation," Tower's data may be converted into a scattergram which yields its coefficient of homogamy. These relationships are illustrated by Table 2.

TABLE 2

RESULTS OBTAINED BY DIVIDING PERCENTAGE OF "CLASS OF MALES" BY THE PERCENTAGE "CLASS OF NORMAL RANGE VARIATION"
(Tower's data)

Class	1	2	3	4	5	6	7	8	9	10
Male	50	100	300	600	1500	4000	1700	1000	600	150
Female	61	91	321	837	1731	3199	2831	746	41	145

On the basis of this reasonable assumption these measurements have recently been recalculated by Willoughby and Pomerat (6) (1932), and a product-moment coefficient of .89±.01 obtained.

Tower infers that, by the elimination of extremes, this process acts to keep the species as close as possible to the mode or standard, so that the population constantly tends toward mediocrity. While it is quite evident that the frequency of mating is markedly low for extremes in size, it is also true, nevertheless, that homogamy in those zones really occurs. It is logical to assume that the genetic identity of these abmodal individuals would be retained if like really tended to mate with like. Given these conditions, there seems to be no indication of a tendency for extremes in size to merge back into the mean distribution value, but rather to remain constant and perhaps even represent potential segregative genetic strains.

Working with the lady-bird beetle *Hippodamia convergens,* Kellogg (2), in the same year, published a note on the number of spots present on mated pairs. From this series of 54 couples he concludes that pairing is entirely at random with respect to color pattern. Pearl (3) (1907) states, in a bibliographical note on Kellogg's paper, that the latter's conclusions are in direct contradiction to the statistics presented.

On the somewhat doubtful assumption that the number of spots may be taken as a continuous variate in the statistical sense, and using a symmetrical table to allow for the fact that the data reported do not distinguish the sexes, a coefficient of correlation of .32±.08 was determined by Willoughby and Pomerat. See Table 3 for Kellogg's data.

TABLE 3
MATING CONDITION OF 54 PAIRS OF *Hippodamia convergens*
(KELLOGG'S DATA)

No. of cases	Condition (no. of spots)
30	Twelve with twelve
13	Twelve with none
5	None with none
2	Twelve with two
2	One with none
1	Twelve with ten
1	Two with none

Since, from the standpoint of the recorded data, these results, though highly suggestive, do not appear to be very definite, it was thought advisable to re-examine the phenomenon of homogamy in a representative insect.

The writer takes this opportunity to express his thanks to Dr. R. R. Willoughby for criticisms and valuable suggestions; to Mrs. Russell Winslow and Miss Edith Fishman for indispensable technical assistance.

II

One hundred twenty-seven mating pairs of the Japanese beetle, *Popillia japonica* Newm. were collected at midday on July 12, 1931, in the New Jersey pine barrens three miles to the southeast of Jenkins, N. J. The area was burned-over territory covered by a scrub growth. The infestation on the wild grape, *Vitis labrusca* L., was especially heavy. Many specimens were collected from the sassafras, *Sassafras variifolium* (Salisb.) Ktze., while the blueberry, *Vaccinium corymbosum* L., and scrub oaks were relatively free. The entire collection was made within one and one-half hours by two workers. Two beetles, one superposed and clasping the other, were considered a mating pair. Whenever any doubt arose concerning the certainty of this matter the individuals were released. Each pair was placed in a one-dram homeopathic vial and stoppered by means of a cork. Within four hours, approximately 4cc. of 10% aqueous formalin was introduced into each vial.

III

In preparation for measurement mating pairs were separated into individual vials which were numbered serially, males receiving odd numbers. All measurements were derived from microscopic drawings; the apparatus used consisted of a compound microscope with a Leitz micro-photography attachment. The projector was essentially that which is employed in micro-photography except for the fact that the image was focused on a piece of rice paper placed on a glass which rested over an opening in a board directly above the camera. By means of this set-up, it was possible to trace the outline of desired structures without changing the focus. The position of the base of the microscope and its tube length were kept constant in relation to the drawing table during the entire course of the work.

Six outline drawings were made of each pair as follows (magnifications may be best estimated from outline drawings):

1. Low-power view of male and female right fore-tibiæ and tarsi (Figures 1 and 2);
2. High-power view of male and female distal end of right tibia, showing spur (Figures 3 and 4);
3. Low-power view of whole ventral surface, male and female (Figure 5).

Eight measurements were taken from these outlines and designated *A, B, C, D, E, F, G,* and *H* (see diagrams). In every case the maximum distance was used as the means of obtaining the points for measure. All cases involving injury to the structures under study

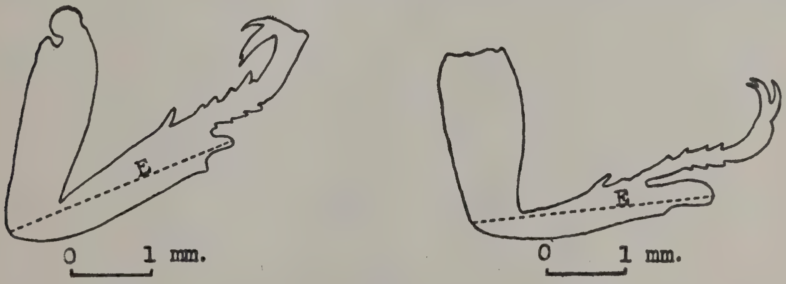


FIGURE 1

FIGURE 2

Example of Outline Drawings Used in Obtaining Data

Interrupted lines indicate measurements taken. Figure 1 is the male and Figure 2 the female fore tibia and tarsi.

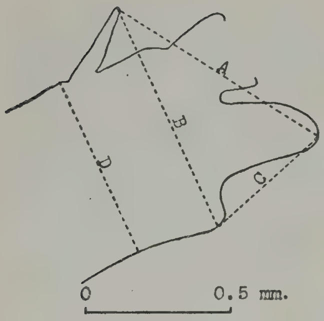


FIGURE 3

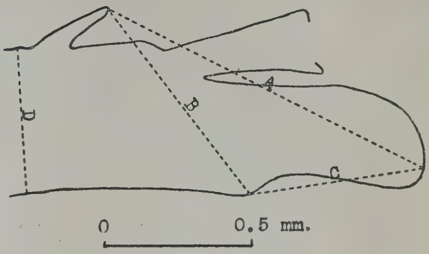


FIGURE 4

Examples of Outline Drawings Used in Obtaining Data

Interrupted lines indicate measurements taken. Figure 3 represents the male and Figure 4 the female distal spur of the right tibia.

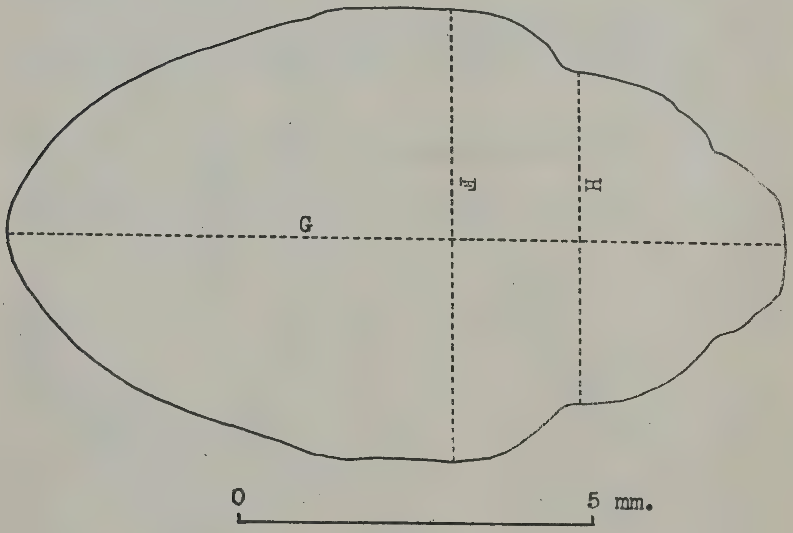


FIGURE 5

Example of Outline Drawing Used in Obtaining Data

Interrupted lines indicate measurements taken. This figure represents the outline of the whole insect (male).

were discarded. Readings were made with a steel millimeter rule to the nearest millimeter.

The distribution of direct measures and morphological indices (these are recorded in millimeters directly from outline drawings without consideration of magnification) are as shown in Table 4.

TABLE 4
Distribution of Direct Measures

Measurement A

	37	39	41	43	45	47	49	51	53	55	57	59	61	63	65	67	69
m	1	1		3	3	16	15	20	29	12	9	1		1	2		
f												1			3	6	9

	71	73	75	77	79	81	83	85	87	89
m										
f	23	16	19	15	11	4	3	2		1

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	113	51.42	4.32	8.5	37.5
f	113	73.98	4.72	6.3	

Measurement B

	38	40	42	44	46	48	50	52	54	56	58	60	62
m	1	2	3	11	18	15	34	16	9	4	1		
f				1	3	7	26	27	29	10	7	3	1

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	114	48.92	3.7	7.56	8.1
f	114	52.58	3.2	6.08	

Measurement C

	22	24	26	28	30	32	34	36	38	40	42
m	4	14	3	40	29	1					
f		2		1	14	31	47	17	7	2	1

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	122	27.3	2.16	7.9	20.2
f	122	33.5	2.66	7.9	

Measurement D

	24	26	28	30	32	34	36	38	40	42	44	46	48
m					4	9	27	23	40	14	2		1
f	4	14	47	34	13	4		1	2		1		

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	120	38.37	2.78	7.2	25.3
f	120	29.25	2.96	10.1	

Measurement E

	35	37	39	41	43	45
m	3	17	32	42	20	8
f	1	8	42	44	20	7

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	122	40.36	2.32	5.7	.74
f	122	40.56	2.02	4.9	

Measurement D

	66	68	70	72	74	76	78	80	82	84	86	88	90	92	94	96	98
m	1	3	3	14	14	18	22	22	16	9	3	1					
f			1	2	3	6	12	15	19	14	19	13	13	5	3		1

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	126	77.64	4.34	5.5	10.7
f	126	83.74	5.06	3.0	

Measurement G

	122	124	126	128	130	132	134	136	138	140	142	144	146
m	1	1	2	6	5	6	7	16	10	14	9	14	11
f			2				1		3	3	12	11	13

	148	150	152	154	156	158	160	162	164	166	168
m	11	4	1	4	3	1					
f	10	14	19	8	3	6	5	3	8	2	3

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	126	140.34	7.3	5.2	10.5
f	126	150.44	8.06	5.3	

Measurement H

	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64
m	1	1	3	3	10	5	17	14	14	13	11	11	10	6	5
f				1	1	1	5	2	6	6	18	13	15	14	14

	65	66	67	68	69
m	1				
f	10	7	5	4	3

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	125	58.19	3.19	5.3	9.8
f	125	62.12	3.28	5.2	

TABLE 5
Morphological Indices

A/C

	135	140	145	150	155	160	165	170	175	180	185	190	195
m	1		1	1	2	4	2	13	11	14	13	11	4
f											1	3	2

	200	205	210	215	220	225	230	235	240	245	250
m	15	7	2	2	3	3	1		2		
f	15	14	19	9	16	10	4	4	8	2	2

	255	260	265	*N*	*M*	*S.D.*	*V*	$d_M\sigma_d$
m				112	187.20	19.05	10.1	12.9
f	2		1	112	216.62	15.6	7.2	

B/C

	125	130	135	140	145	150	155	160	165	170	175
m					2	5	6	5	11	17	21
f		3	8	10	22	15	12	10	14	6	4

	180	185	190	195	200	205	210	215	220	225	230	235
m	11	12	6	3	6	1	3		3			1
f	3	3		2	1							

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	113	177.00	16.70	9.4	11.7
f	113	154.75	14.65	9.7	

E/C

	103	106	109	112	115	118	121	124	127	130	133	136
m									1	4	3	4
f	3	6	4	19	16	15	22	8	6	5	7	3

	139	142	145	148	151	154	157	160	163	166	169	172
m	18	17	17	13	9	7	11	6	4	2		2
f	2	3						1				

	175	178	*N*	*M*	*S.D.*	*V*	d_M/σ_d
m		2	120	147.52	9.9	6.7	22.1
f			120	119.89	9.51	7.9	

E/D

	84	88	92	96	100	104	108	112	116	120	124	128
m	1	4	1	4	30	34	29	10	5			
f						2	1			1	5	10

	132	136	140	144	148	152	156	160
m								
f	10	15	36	15	16	5	1	1

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	118	104.07	5.88	5.6	37.8
f	118	141.52	9.16	6.4	

B/E

	104	106	108	110	112	114	116	118	120	122	124	126
m	3		3	4	6	10	12	12	14	16	9	9
f							3	4	9	7	14	11

	128	130	132	134	136	138	140	142	144	146	148
m	5	4		1	1	1	1	1	1		
f	15	15	7	7	9	5	2	1	2	1	1

N	*M*	*S.D.*	*V*	d_M/σ_d
113	120.10	7.32	6.09	9.2
113	128.51	6.72	5.20	

H/G

	360	365	370	375	380	385	390	395	400	405	410
m	1	1	2	1	1	8	5	12	9	10	12
f				3	1	2	7	11	14	16	18

	415	420	425	430	435	440	445	450	455	460	465
m	15	23	2	10	4	3	2	1	1	1	1
f	12	12	12	8	5	2	1	1		1	1

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	125	410.50	18.85	4.5	.02
f	125	410.55	14.95	3.6	

F/H

	124	126	128	130	132	134	136	138	140	142	144	146
m	1	1	2	17	30	36	24	11	1			
f	2	1	3	5	19	28	33	29	4	1		

	148	150	152	154	156	158
m			1			1
f						

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	125	133.86	3.70	2.7	2.37
f	125	134.88	3.18	2.3	

G/H

	214	217	220	223	226	229	232	235	238	241	244
m	1	2	1	1	4	5	10	16	19	11	13
f			1	2	3	5	16	8	12	22	15

	247	250	253	256	259	262	265	268	271	274
m	11	12	6	6	3	1	1	1		1
f	10	14	10	3	1	1	2			

	N	*M*	*S.D.*	*V*	d_M/σ_d
m	125	241.42	9.12	3.7	.81
f	125	242.23	6.24	2.5	

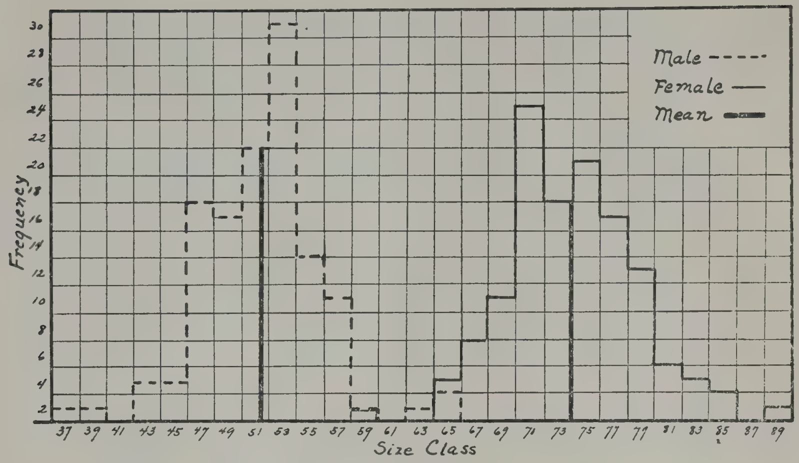


FIGURE 6
Histogram of Direct Measure
Frequency *vs.* size class. Example used is measurement *A*.

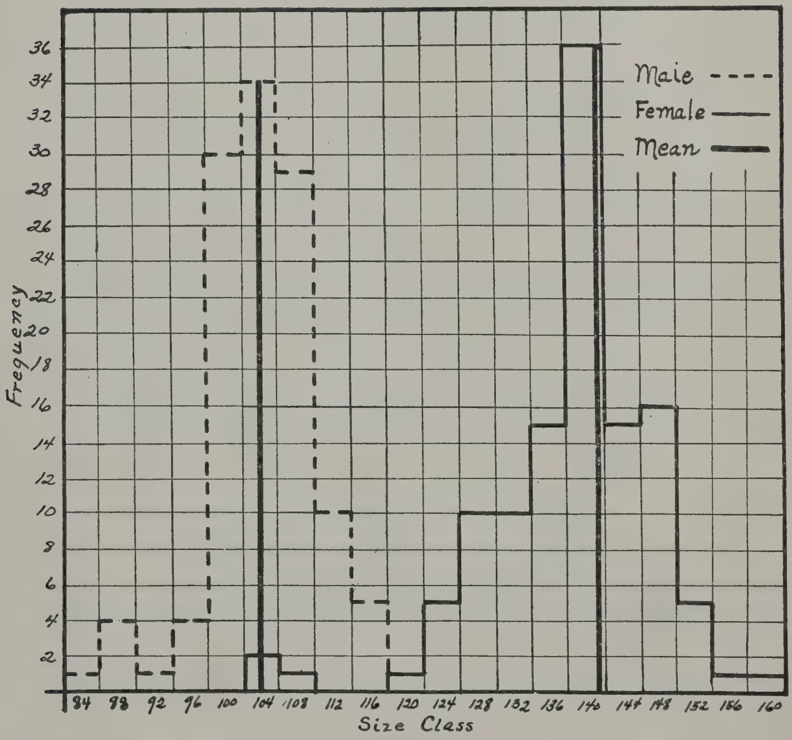


FIGURE 7
Histogram of a Morphological Index
Frequency *vs.* size class. Example used is *E/D*.

The accompanying histograms illustrate the data presented above in two selected cases, Figure 6—direct measure, male A with female A; Figure 7—morphological index, *E/D*.

NOTE: Calculation of the d_M/σ_d was not carried through since the values obtained ranged from about 0.5 to 1.5 and were therefore considered too low to be significant.

Table 6 is a summary of the data presented above showing size and form variations in both male and female.

TABLE 6

SUMMARY OF MEASUREMENTS SHOWING IN PERCENTAGES TO WHAT DEGREE ONE MEMBER OF THE PAIR IS GREATER THAN THE OTHER

Direct Measurements:		
Greater size		
	Male	D—23.5%
	Female	A—31%; B—7%; C—17%; E—4% F—6%; G—6%; H—6%
Greater variability		
	Male	A—26%; B—19.5%; E—14% F—45.5%; H—2%
	Female	D—28.5%; G—2%
Morphological Indices:		
Greater size		
	Male	B/C—12.5%; E/C—19%
	Female	A/C—13.5%; E/D—26%; B/E—6.5% H/G—.001%; F/H—.5%; G/H—.5%
Greater variability		
	Male	A/C—28.5%; B/E—14.5%; H/G—20% F/H—15%; G/H—32.5%
	Female	B/C—3%; E/C—15%; E/D—12%

(Example interpretation: In the direct measurements—in measurement *D*, the male was, on the average, about 23.5% larger than the female; in all other measurements of size, the female was larger.)

Pearson product-moment correlations were obtained for some of the direct measures and morphological indices. These are summarized in Tables 7 and 8.

IV

From Tables 7 and 8 it will be seen that the coefficients of correlation are, in general, very low, especially in view of the fact that the mean probable error is .06. The distribution of the correlations of direct measures group themselves about a mean of approximately .044. The highest correlation was found to exist between the width of the male tibia and the length of the female tibial spur (♂*D*—♀*C*). The lowest correlation was obtained between the width of the male tibia against an oblique line running from the base of the outer margin of the spur to the end of a prominent spine on the opposite side of the female tibia (♂*D*—♀*B*) (see Figure 4). Both of these

TABLE 7
DIRECT MEASUREMENTS

	A	B	C	D	E	F	G	H
A	.05	.05	—.03	—.06	—.12			
B	—.04	—.06	—.28	—.41	—.20			
C	.09	—.01	.03	.57	—.17			
D	—.01	.15	.14	.01	.08			
E	.19	.14	.03	.04	.12			
F						.08	.14	.12
G						.08	.19	.03
H						.21	.18	.18

Male—horizontal axis
Female—vertical axis

M=.044±.06
+=67%
—=33%

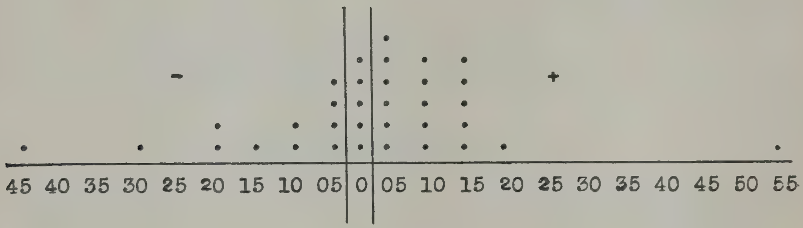
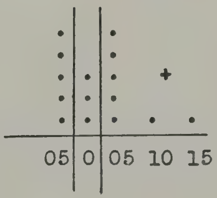


TABLE 8
MORPHOLOGICAL INDICES

	A/C	A/E	B/C	B/E	E/C	E/D	H/G	G/H	F/H
A/C	.06								
A/E		.02							
B/C	—.01		.06			.05			
B/E				—.01					
E/C	.01				.05				
E/D			—.03			—.02			
H/G							.15		
G/H								.09	—.03
F/H								.12	.004

Male—horizontal axis
Female—vertical axis

M=.034±.06
+=67%
—=33%



measurements were derived from structures magnified at the same power (63 x — highest magnification studied). The $\frac{D}{\sigma_d}$ of the lowest value (—.41) is 3.03, and therefore shows true negative homogamy. The high correlation (.57) with $\frac{D}{\sigma_d}$ of 4.2 is the only significant measure of positive homogamy, but the measurements involved do not seem to have any great mechanical import with respect to the mating act. In an attempt to check the possibility that a spurious correlation was being dealt with, an artificial correlation was set up in which the males were correlated, not with their natural mates, but with the female of the next pair in the series. This controlled pairing yielded a correlation of .11; with a twice removed "mate," .01; with a thrice removed "mate," —.03; and with a "mate" removed four times in the series from the true mate, the correlation obtained was —.17. When repeated for the low extreme value (—.41), this technique gave .15 for the once removed "mate"; when twice removed, .004. For a coefficient of typical median value (.04—♂*D*—♀*E*), this method gave —.03. Since these controlled pairings gave correlations which grouped themselves about the mean value, the average being .011, this furnishes additional evidence that the extreme values are not spurious, i.e., that there is no intrinsic factor which would direct positive or negative correlations respectively.

Heterogeneous linear measurements yield variable coefficient values and possibly in some species the range of this variability is not great. This is undoubtedly true in man, while in *P. japonica* the scatter appears wide. Does the significant technique for coefficients of homogamy then depend upon the variability of a group of linear correlations? This work suggests that a more refined technique should involve pooled data arranged in such a way as to show this relation to a normal distribution curve. Such a technique, however, may not be necessary in cases where a discrete character upon which the mating act depends can be isolated (as in the conjugation phenomenon exhibited by Paramecium).

It will be noted that gaps have been left in both Tables 7 and 8. These gaps were considered justifiable because the general trend of the distribution did not seem to indicate that more calculated data would alter the outline of the picture already formed.

From the relatively few calculated correlations of the many possible, a distribution similar to that of the direct measures was found for the morphological index correlations, the mean value being about .01 less than that in the former (.034). The highest value for the group (.15) was found to exist between the ratio of the thoracic width to the total length in the male (*H*/*G*) against the same ratio in the female. Doubtless, this is the most crucial correlation since it involves the greatest length and the most dependable width measurement in this organism (abdominal width in the female might vary as a funtion of egg content). Furthermore, the *P.E.* (.06) in this calculation gives added indication that homogamy in the Japanese beetle is very low.

V

An attempt has been made in this study to obtain a series of measurements on a fairly common and markedly aggressive, introduced Coleopteran in an attempt to gain sufficient data to arrive at an appreciation of the character of mate selection in a representative insect. The work has included the obtaining of eight linear measurements on mated animals. These values were obtained by means of a standard technique in which outlined drawings obtained by microprojection gave permanent records. Measurements were taken at three different magnifications. These included the outstanding key characteristic for the differentiation of sex, namely, the fore tibia, which bears a spur quite different in both sexes. In addition, measurements of larger structures were taken, i.e., total length, thoracic width, and abdominal width. These data were studied to obtain correlations of direct measures and correlations for morphological indices (Pearson's product-moment correlation). Table 6 summarizes the significant differences between the sexes as found by an examination of Tables 4 and 5. It will be seen from the table that the female is usually larger in size than the male, with a notable exception in the fore tibial width (*D*). On the other hand, the female is less variable both in direct size and form than the male, with a significant exception in its fore tibial width (*D*).

A mean homogamy value for direct size of .044±.06 and a mean value for form of .034±.06 were obtained. Distribution plotting of indivdual values in both cases shows fairly normal distribution with mean values slightly above zero. There seems to be, then, little difference between the findings of these two classes of correlations from the standpoint of mean values.

It has been indicated by workers on invertebrates that high values for homogamy might be expected in organisms whose relatively inflexible endo-skeletons did not permit adjustment of clasping mechanisms and structures specialized to effect fertilization. This was found to be true particularly in the crustacean *Gammarus,* Crozier (1) (1923). On this basis, a high homogamy in *Leptinotarsa* might have here been a reasonable expectation.

The findings in *Popillia,* then, show that the degree of homogamy is not so uniform in invertebrates as earlier studies appeared to indicate. In a previous paper by Willoughby and Pomerat (6) (1932), it was suggested that the high values characteristic of the published reports on invertebrate homogamy might imply a relatively high rate of specific differentiation for the sub-phylum; the present results tend to invalidate this hypothesis by throwing doubt upon the general occurrence of these high homogamy values.

A wide range of correlation values based on heterogeneous linear measurements was found in the course of this study, indicating that no single measure can be relied upon to yield a representative index for the degree of homogamy present; the central tendency of a distribution of coefficients derived from different measures is more likely to yield such an index.

These results indicate that much more systematic work must be done before a satisfactory theory can be formulated to aid in the interpretation of the significance of selective mating in invertebrates. With this end in view, further studies on the Coleoptera have been started in this laboratory.

It is not improbable that coefficients of intermediate values between the high ones reported by earlier workers and that which is given here may be found. If this were true, it is possible that there may exist a relationship between the degree of homogamy and the number of species and sub-species within a given genus on the basis that, with high homogamy, extreme variates tend to segregate. This can probably be demonstrated theoretically by an artificial population technique such as that used by Willoughby and Goodrie (5) in a study of the differential birth rate problem.

REFERENCES

1. CROZIER, W. J., & SNYDER, L. H. Selective coupling of Gammarids. *Biol. Bull.*, 1923, **45**, 97-104.
2. KELLOGG, V. L. A note on assortative mating. *Science*, 1906, **24**, 665-666.
3. PEARL, R. A biometrical study of conjugation in *Paramecium*. *Biometrika*, 1907, **5**, 213-297.
4. TOWER, W. L. An investigation of evolution in Chrysomelid beetles of the genus Leptinotarsa. *Publs. Carnegie Instit.*, Washington, No. 48. Pp. 320.
5. WILLOUGHBY, R. R., & GOODRIE, M. Some neglected factors in the differential birth rate problem. *J. Genet. Psychol.*, 1927, **34**, 272-393.
6. WILLOUGHBY, R. R., & POMERAT, C. M. Homogamy in the toad. *Amer. Natur.*, 1932, **66**, 223-234.

Clark University
Worcester, Massachusetts

L'ACCOUPLEMENT CHEZ LE SCARABÉE JAPONAIS *POPILLIA JAPONICA* NEWM.

(Résumé)

On s'est servi de Popillia japonica dans une enquête pour trouver le degré de l'accouplement sélectif chez un insecte type. Au moyen d'une technique étalon où l'on a employé des dessins de contour obtenus par la micro-projection, on a obtenu huit mesures linéaires de quelques animaux accouplés. On a computé de ces données des corrélations Pearson produit-moment des mesures directes et des indices morphologiques. A l'exception d'une cas, on a trouvé que la femelle a été plus grande que le mâle; mais qu'en général, la femelle a été moins variable en grandeur et en forme que le mâle. On a trouvé une valeur moyenne d'homogamie de 0,044±0,06 pour la grandeur et une valeur moyenne de 0,034±0,06 pour la forme. Ces résultats chez Popillia montrent que le degré de l'homogamie n'est pas si uniforme chez les invertébrés que les études antérieures semblent l'indiquer. On a trouvé une variation des valeurs directes de corrélation s'étendant de —0,41 à +0,57, basée sur des mesures linéaires hétérogènes. Cette grande variation indique qu'aucune seule mesure n'est certaine de donner un indice représentatif du degré de l'homogamie qui existe; la tendance centrale d'une distribution des coefficients dérivés des mesures différentes est plus capable de donner un tel indice.

POMERAT

DIE BEGATTUNGSTÄTIGKEIT BEI DEM JAPANISCHEN KÄFER (POPILLIA JAPONICA NEWM.)

(Referat)

Es wurde eine Untersuchung an Popillia japonica durchgeführt in der Absicht, den Grad der Auslesung bei der Begattung (degree of selective mating) bei einem typischen Insekt festzustellen. Unter Gebrauch eines vorschriftsmässigen Verfahrens mit Verwendung von Umrisszeichnungen

die durch Mikroprojection erhalten worden waren wurden acht Linienmessungen an gepaarten Tieren erhalten. Es wurden dann an diesen Daten Pearsonsche Produkt-Momentkorrelationen von direkten Messungen und von morphologischen Indizien berechnet. Mit einer Ausnahme zeigte es sich dass die Weibchen grösser waren als die Männchen, dass die Weibchen aber im Allgemeinen aber weniger variabel waren als die Männchen, in Bezug sowohl auf Grösse wie auf Form. Man fand für die Homogamie (homogamy) in Bezug auf Grösse einen Durchschnittswert von .034±.06. Diese Befunde bei Popillia erweisen, dass der Grad der Homogamie bei wirbellosen Tieren nicht so genau übereinstimmt wie frühere Untersuchungen anzudeuten schienen. Man fand eine Ausdehnung der direkten Korrelationswerte die sich von —.41 bis +.57 erstreckte. Diese Werte stützen sich auf heterogene Linienmessungen. Diese weite Ausdehnung weist darauf hin, dass man sich bei der Nachforschung nach einem vorbildlichen Index des Grades der gegenwärtigen Homogamie auf einen einzelnen Massstab nicht verlassen kann. Die zentrale Neigung der Verbreitung von Koeffizienten die an verschiedenen Massstaben berechnet worden sind ist zur Lieferung eines solchen Indexes günstiger.

POMERAT

A FURTHER STUDY OF GRASPING*

From the Clinic of Child Development, Yale University

H. M. Halverson

Introduction

Our purpose in this investigation is to study the nature of the development of grasping in infancy and to discover the factors which determine the course of the development. The paper concludes a four-year investigation of prehension of objects of different forms by infants who range in age from 4 to 60 weeks. The infants are examined regularly at four-week intervals, according to a normative program of study (12, p. 305), and their prehensory reactions to the objects are recorded on a motion film.[1]

We know that at successive stages in development certain portions of the hand which at first are most concerned in prehension become less and less active, while other portions which at first take only a passive part in prehension gradually take on more of the responsibilities involved in the acquisition of objects (13). The writer will indicate and describe these evidences of shift in prehensile activity and inquire into the neurological, physiological, and psychological forces which underlie these very palpable changes. He believes that there are very fundamental reasons why some parts of the hand function in early prehension and other parts cooperate in mature reaching and grasping activities.

One finds considerable general information[2] on the subject of prehension, in which the investigators describe those reactions which one observes with the unaided eye. Such observations, of course, preclude accuracy in report of details and are confined to fragments of the infant's total response. The response in a given situation usually consists of many coordinating activities by several parts of the body, which start and run their courses simultaneously. In time these activ-

---

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, January 4, 1932.

[1] For manner of presentation of objects, plan of operation of the motion camera and method of study of cinema records, see Halverson (13, p. 122).

[2] If the reader desires a complete list of references on prehension he should consult Castner (5) and Halverson (13).

ities combine into a concerted prehensory movement upon the object of regard, but each activity must be observed separately many times before one can correctly trace its part in the total response (13). An analytic study of infant prehension by eye alone yields meagre and, in some cases, doubtful returns. A faithful recording apparatus which gives an accurate running account of these complicated responses and permits unlimited repetitional study of them is one of the best means of conducting investigations of analytic nature. In the present instance the motion camera serves as the recording device.

An introductory study (13) shows that reaching varies from the very circuitous approaches at 16 weeks in which abductive-adductive shoulder movements principally direct reaching reactions to the straight approaches of the 1-year infant. Their vertical profiles develop gradually from pawing or sliding movements of the hand on the table at 16 weeks to projection of the hand on a low broad arc over the table top at 52 weeks. For the first 6 months of life infants usually reach haltingly for all objects so that raising, projection, and lowering of the hand in many instances appear almost as separate acts. Before one year of age infants have combined these three acts into a smooth approach which defies resolution into its component parts. In the case of the younger infants the hand is a reflex grasping tool at the end of a directing upper arm and moves as the latter moves. Perhaps this fact explains the rather jerkily executed hand movements of early infancy. With older infants the index finger and thumb lead in reaching and grasping movements (13, 27).

An interesting development in reaching is the change in the functional relationship of forearm and hand. During the first 6 months the hand of the infant moves about, paw-like, in so fixed a manner that this member appears to be only a distal hinge of the forearm. Observation of many infants shows that early in life the forearm and hand lie in a straight line (13, p. 201) (see Figure 2, *Aspects of Hand Development,* 16). At one year the hand so flexes ulnarward at the wrist that the elbow-wrist line extends out to a point midway of the thumb and index finger (see Figure 2, *Aspects of Hand Development,* 56). Wrist flexion, ulnarward, occurs after 6 months of life and is undoubtedly the direct result of changes in digital adjustment in grasping. Older infants grasp by the tips of the digits. Of all the digits the thumb and index finger are of greatest importance in prehension, and it is natural that infants in reaching set these digits in a manner which is advantageous for grasping. Thumb abduction and ulnar flexion of the hand at the wrist which carries the fingers out laterally expose the volar surfaces of the two radial digits and bring them forward in preferential relationship to the object for which they are reaching. Therefore, in reaching, the forearm of the 1-year infant from elbow to wrist often points straight toward the object, while the median line of the hand points in a lateral direction. Now the hand of the younger infant resembles a paw not only in its physical relationship to the forearm but in its reactions. The hand paws the table, digs at it in scratching and raking movements, corrals objects toward the trunk by inward scooping motions and joins the other hand in pressing and holding objects against the table top. No digit is outstanding in early prehension, although the medius, by virtue of its midposition and length, often appears to advantage in both reaching and grasping.

In his study of cube prehension the writer outlines seven definite stages in the development of grasp (13). Inasmuch as they form the basis for this study, he presents a brief review of these stages.

In the *ulnar* or *squeeze grasp* the three ulnar digits flex on the object and press it against the proximal palm. In the *hand grasp* all fingers press the object against the palm. In the *palm grasp* the thumb for the first time so pivots about that it opposes the remaining digits. The index finger now asserts its leadership in all prehensory activity. In the *superior-palm grasp* the three radial digits principally with thumb opposition hold the object within the radial palm. In the *inferior-forefinger grasp* the index and medius fingers, sometimes accompanied by the ring finger, press the cube against the thumb. In the *forefinger grasp* the cube is held well out on the digital tips with thumb opposition. In the *superior-forefinger grasp* the infant plucks the cube from the table top by the tips of the digits without stopping for adjustment of any sort.

The Experiment

The objects which are used in this experiment are: a hollow rubber ball, a crayon, a cube, a cup, a hand bell, a pellet, a rattle, a short rod, a teaspoon, and a two-strand twisted string. The dimensions, weights, and colors of the objects and the ages at which they are presented appear in Table 1.

The cube, crayon, cup, hand bell, pellet, and spoon are placed

TABLE 1

SHOWING SIZE, WEIGHT, AND COLOR OF THE OBJECTS AND THE AGES AT WHICH EACH OBJECT IS PRESENTED

Object	Size (inches)	Weight (ounces)	Color	Age presented (weeks)
Ball	2.5 diameter	2.50	white	36-60
Crayon	4.5 length x 0.5 diameter	1.00	red	36-60
Cube	1.0 edge	0.25	red	12-60
Cup	3.8 top diameter x 2.4 height	4.00	white	16-60
Hand bell	3.5 height x 2.4 length of handle	1.00	black handle	16-60
Pellet	0.3 horizontal diameter	. . .	white	16-60
Rattle	2.8 diameter of ball	0.50	white	4-28
Rod	4.1 length x 0.4 diameter	0.25	red	4-20
Spoon	5.8 length	0.25	aluminum	16-60
String	11.0 length	. . .	white	16-60

singly on the table before the seated infant;[3] the ball is rolled to him; the string with a ring at its remote end is place obliquely on the table so that the near end of the string is 7 inches directly ahead of his right hand at the table edge. If, at the end of 20 seconds, the infant does not procure the string, it is re-presented in the midline of the table so that the string now is but 5 inches from the infant. The rattle and rod are put in the hand of the dorsal infant.

The study actually covers grasping of eight objects, although ten articles appear in the table. Inasmuch as one presents the ball only to infants of 36 to 60 weeks of age, one substitutes the rattle for the ball in the earlier ages, because the spherical end of the rattle is of a diameter comparable with the diameter of the ball. For a similar reason, the handle of the rattle and the crayon replace the rod at ages where the latter is not presented. The results apparently warrant these substitutions.

These eight objects differ so greatly in size, weight, and form that they should elicit about all the types of grasping which infants of the ages which are represented in this investigation have in their prehensory repertoire. For example, the pellet of negligible weight has its center of gravity so low that its prehension requires very fine coordination of eye and digits. The ball is extremely large for an infant and calls for gripping by widely separated digits. The cup which is usually grasped by the rim or handle has its center of gravity outside the hand so that a new hazard in holding presents itself. The string lies so close to the table top and is of so small a diameter that it easily evades all but the most carefully placed digits. The spoon and the bell are irregular in form and evoke a variety of grasps. Of all the objects the rod and the cube are, perhaps, most easily manipulated. One can easily see that prehension of the above-mentioned objects is most likely to elicit numerous types of grasping.

Seven infants participate in the experiment. None of them is missing at any age, except at 12 weeks and 52 weeks when the number of infants is six, and at 56 weeks and 60 weeks when the number is five infants.

METHOD OF STUDY

The method of study combines visual observation through a one-way visibility screen with systematic motion and frame-by-frame analysis of cinema records of the reactions of infants to the different objects which are presented by the examiner. In this way one slights neither the flow of action nor particulars of mechanics of movement. A study of grasping by infants who exhibit prehensory reactions which range from the earliest reflex clasp to the highest forms of closure by adults should impart information concerning prehensory predispositions of man, order of digital development in grasping, progressive changes in the position of the point of converging oppositional forces in holding, and possibly the manner of development of cutaneous sense perception and motor skill.

---

[3] A full description of the table top and the placement of the infant with respect to the table top appears in an earlier study (13).

Discussion of Results

In this study one lists only those grasps by which the infant secures objects from predetermined positions on the table. Grasps which occur when objects are displaced from position or are transferred from hand to hand are generally accomplished under difficulties which necessitate recourse to a lower type of hold than infants of a given age usually apply to objects which are easily accessible (13, p. 221).

Grasping of the cube, rod, spoon, cup, and bell permits of classification under the above categories (see p. 40), while grasping of the ball, pellet, and string requires special classification.

One experiences no difficulty in elicitation of grasping responses to objects. The presence of an object within visual and reaching distance of an infant seldom fails to release the mechanism of prehension.

Cube, Rod, Spoon, Cup, and Bell[4]

Table 2, *Cube,* indicates that ulnar grasping (Figure 1, *Cube 20*) appears early in life and functions strongly up to 24 weeks. Hand grasping (Figure 1, *Cube 24*) occurs generally from 16 to 28 weeks; palm grasping (Figure 1, *Cube 32*) and superior-palm grasping (Figure 1, *Cube 36*) occur often from 28 to 40 weeks; inferior-forefinger grasping (Figure 1, *Cube 48*) appears at 32 weeks and plays an important part in grasping throughout the remaining ages; forefinger grasping (Figure 1, *Cube 56*) begins at 40 weeks and from 52 to 60 weeks is the usual type of grasping. Superior-forefinger grasping (Figure 1, *Cube 56*) occurs frequently at 52 and 56 weeks, and is the type of grasping which adults utilize in all forms of prehension.

---

[4] The drawings of Figures 1 and 2 were made by Miss Glenna Bullis from original photographs.

TABLE 2
FREQUENCY OF OCCURRENCE IN EACH AGE GROUP OF THE SEVERAL TYPES OF GRASPS ON THE EIGHT OBJECTS

The age groups are listed at the top and the types of grasp in the extreme left and extreme right columns of the table.

Type of Grasp	Age in Weeks													Age in Weeks													Type of Grasp
	12	16	20	24	28	32	36	40	44	48	52	56	60	12	16	20	24	28	32	36	40	44	48	52	56	60	
Sup.-Foref.									1		7	7	3														Sup.-Foref.
Forefinger		Cube					1	6	12	11	18	12	13		Rod						2	2	2	5	7	9	Forefinger
Inf.-Foref.					1	22	19	18	20	20	11	12	8						3	6	8	10	10	8	11	9	Inf.-Foref.
Sup.-Palm		1		6	19	10	16	9	4	5	1	2						2	2	11	5	5	2	2	1	1	Sup.-Palm
Palm				1	6	4		5	6	5			1		5	6	10	6	8	7	8	7	6	3	1		Palm
Hand	1	7	12	21	11	7	7	4		1		1		4	1	1											Hand
Ulnar	5	9	8	8	3									7	3	3											Ulnar
Sup.-Foref.																							2	4	4	1	Sup.-Foref.
Forefinger		Spoon								1	7	3	6		Cup												Forefinger
Inf.-Foref.					1	5	6	5	6	9	6	7	6						4	6	7	6	7	8	6	8	Inf.-Foref.
Sup.-Palm						4	5			1			1					1	2	2	3	4					Sup.-Palm
Palm				3	5	4	8	4	3	3		2	1			1	3	3	6	8	5	1	1	2	1		Palm
Hand			3	6	3	1	1										3	2									Hand
Ulnar		2	4	2	1										1	2	5	2									Ulnar
Sup.-Foref.									2	2	5	4	5								2	7	9	9	15	12	150°
Forefinger		Bell								1			1		Ball					3	10	9	11	10	8	7	120°
Inf.-Foref.							6	8	9	9	6	9	6				1	2	3	18	13	7	6	9	4	4	90°
Sup.-Palm						3	4	6	4	4	5	1						2	4	9	1	1	3	3		1	70°
Palm			1	9	9	10	8	5	4	3	1	3	2			1	1	8	9	6	11	4	3			1	50°
Hand				1	2										1	4	9	9	4	3			1			1	30°
Ulnar				1										5	1		8										0°
Sup. Obliq. Pinc.																											Sup. Obliq. Pinc.
Obliq. Pinc.		Pellet							1			4	1		String										2	2	Obliq. Pinc.
Vert. Pinc.							2	8	10	14	9	6	5									2	8	7	7	5	Vert. Pinc.
Scissors							6	6	9	5	2	2							2	5	6	4	5		1		Scissors
Palm					4	9	2	5			1							1	3	2	3	3					Palm
Hand			1	2	2	2											1	7	5	1							Hand
Ulnar																											Ulnar

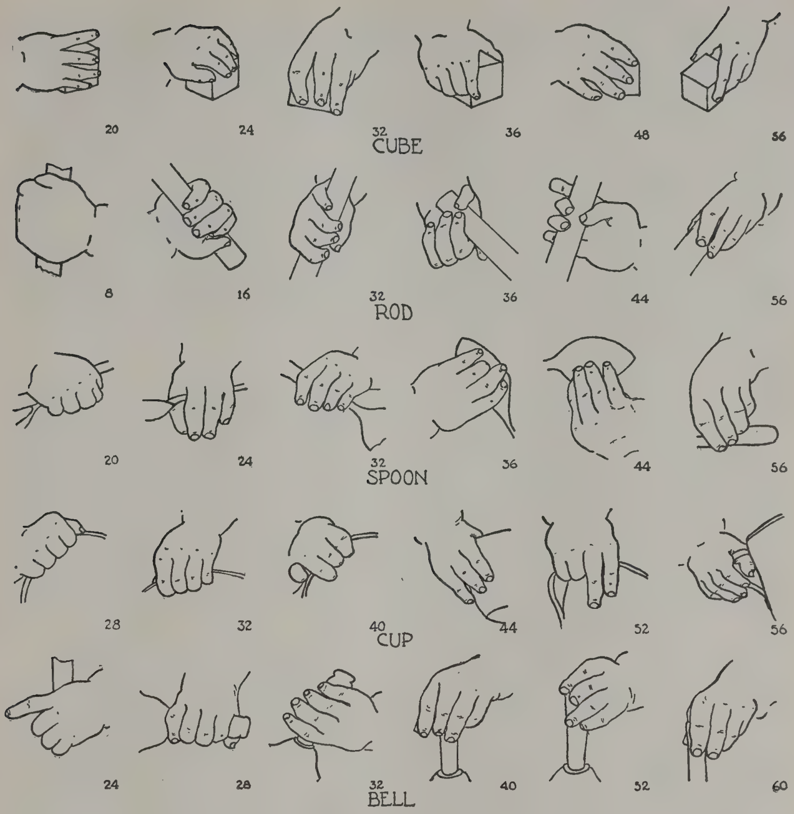

20
24
32
CUBE
36
48
56
8
16
32
ROD
36
44
56
20
24
32
SPOON
36
44
56
28
32
40
CUP
44
52
56
24
28
32
BELL
40
52
60

FIGURE 1
SHOWING A SERIES OF GRASPS IN DEVELOPMENTAL SEQUENCE FOR EACH OF FIVE OBJECTS: CUBE, ROD, SPOON, CUP, AND BELL
The numbers below each drawing indicate the age at which that grasp occurs.

Only one cube is presented to each infant at 12 weeks and this cube is put into the left hand. If the infant does not open his hand for the cube, the four fingers are unclasped by the examiner, and the object is inserted into the palm. At all other ages the examiner presents three cubes on the table top, one at a time, and offers three by hand to the infant.[5] Hence, each infant may register six grasps at each age. Sometimes an infant fails to grasp one or two of the cubes.

---

[5] The reader will find a full description of the manner of selection of infants for the experimentation and of presentation of the cubes in the earlier study (13).

Grasping in the case of the rod begins as ulnar (Figure 1, *Rod 8*), and soon develops into palmar gripping (Figure 1, *Rod 32*). While superior-palm grasping (Figure 1, *Rod 36*) next appears, it soon gives way to inferior-forefinger grasping (Figure 1, *Rod 44*). Forefinger grasping (Figure 1, *Rod 56*) becomes established only at 56 and 60 weeks. Some infants never apply the superior-palm grasp to the rod. In their cases the palm grasp gives way to the inferior-forefinger grasp. For some reason, perhaps because the object is long and slender, the rod elicits more palm grasping than does any other form of object. Grasping by the digits develops later in the rod than in the cube situation.

Table 2, *Spoon,* shows that, with the exception of infrequent appearances of the superior-palm type of gripping, prehension develops gradually from ulnar grasping to forefinger holding. The difference in form of the two long objects, the rod and the spoon, probably accounts for the difference in the rate of prehensory development. The slight height of these objects practically precludes superior-forefinger grasping by infants. Figure 1, *Spoon,* illustrates in order the following grasps: hand, interdigital, palm, inferior-forefinger, second type of inferior-forefinger, forefinger.

Unless the infant grasps the cup by both hands, its center of gravity is always outside the hand. Its great size, mass, shape, smoothness, and projecting handle make the cup a formidable object for grasping by the younger infants. Therefore, infants of all ages usually employ all digits in holding. Infants grasp the cup either by the rim or the handle but only when infants are one year old does the handle actually aid in prehension. According to Table 2, *Cup,* palm grasping and inferior-forefinger grasping predominate. Figure 1, *Cup,* shows from left to right the following types of grasping: hand, palm, superior-palm, inferior-forefinger, superior-forefinger, and a hold on the handle of the cup.

The examiner presents the bell in an upright position on the table. Very young infants usually grasp the bell by its bowl, because this portion of the object is adjacent to the table top and young infants always place the hand on the table in grasping. As infants mature, they place their hands higher and higher on the handle. Table 2, *Bell,* shows that palmar grasping predominates up to 36 weeks, after which time inferior-forefinger grasping occurs most frequently. Superior-forefinger grasping begins near the end of the first year of life.

The illustrations on bell grasping, as they appear in Figure 1, are, in order: hand, palm, palm again, superior-palm, inferior-forefinger, and superior-forefinger grasping.

BALL

Ball grasping does not permit of classification under categories which apply to prehension of smaller objects. The great size of the ball and the absence of opposed surfaces make palmar contact essential at all ages. Classification, therefore, is in terms of the number of degrees of spherical surface which lies exposed radially between the index finger and thumb.[6] Infants of this age sink the ball into the palm so that they may secure oppositional gripping on this large object by the thumb and finger tips. An examination of the cinema records reveals that the tips of the digits "dig" into the surface of the ball.

The amount of surface which subtends the angle of the thumb and index finger increases steadily as infants mature. Table 2, *Ball,* shows the distribution of the amounts of the subtending surfaces in grasping for each of the age groups.[7] Although the range of the distribution for each age is great, the increase in the size of the subtended angle with increase in age of the infants is clearly shown.

The illustrations on ball grasping (Figure 2, *Ball*) indicate the manner in which infants of 8, 16, 24, 36, 44, and 56 weeks hold large objects. Closure at 8 weeks does not include the ball; the fingers often slip by the ball and close against the palm.

From 16 weeks to 56 weeks the ball moves in successive stages from mid-palm to radial palm, and from 36 weeks to 56 weeks the thumb appears in active opposition in holding.

PELLET AND STRING

The small surface of the pellet (5) and the string necessitates fine digital adjustment in grasping, which results in a series of grasps that differ considerably in type from grasps on larger objects.

---

[6] One recognizes the fact that the determination of amounts of spherical surfaces by direct observation and by photographs is subject to grave errors of judgment. However, under the conditions of the experimentation no other expedient suggests itself. Obviously, one cannot interrupt the examinations and actually measure the exposed surfaces.

[7] In constructing Table 2, *Ball,* one selects arbitrarily the steps (in degrees) which indicate the thumb–index-finger angle. Distributions based on other selections show the same general increase with age in amount of surface which subtends the thumb–index-finger angle.

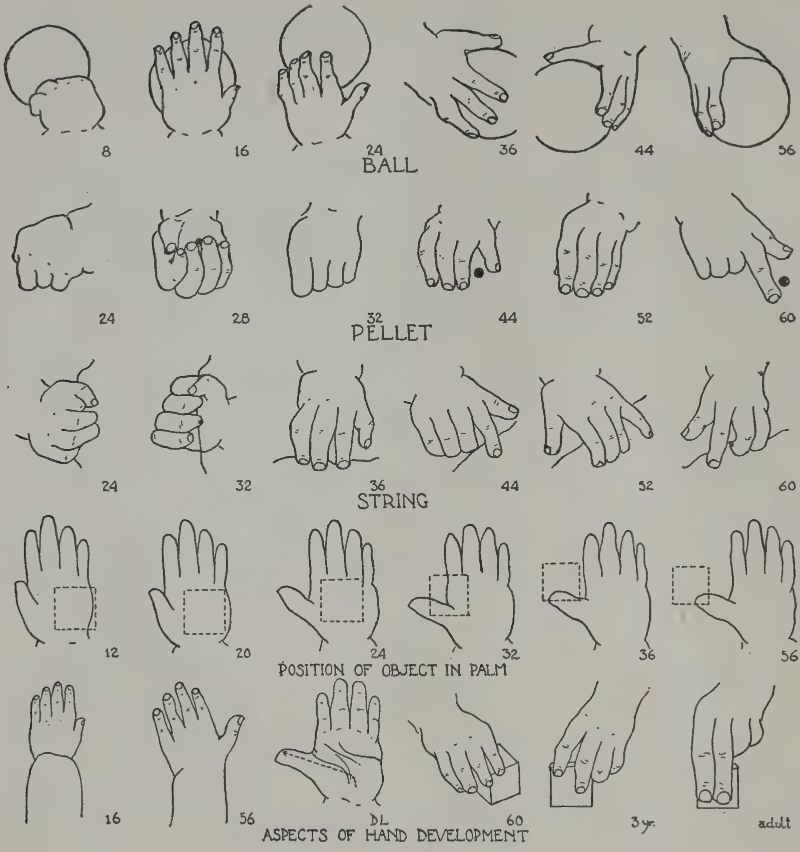

8
16
24
BALL
36
44
56
24
28
32
PELLET
44
52
60
24
32
36
STRING
44
52
60
12
20
24
32
POSITION OF OBJECT IN PALM
36
56
16
56
DL
60
ASPECTS OF HAND DEVELOPMENT
3 yr.
adult

FIGURE 2

Illustrating a Series of Grasps in Developmental Sequence for Each of Three Objects: Ball, Pellet, and String

This figure also shows the six successive positions on the palm against which the fingers press objects, such as cubes. Under the caption, *Aspects of Hand Development,* appear in order: the forearm of a 16-weeks infant, the forearm of a 56-weeks infant, the developmental line (note broken line) of opposition in grasping, and digital grasping by a 60-weeks infant, by a 3-year-old child, and by an adult, respectively.

The first type of closure is *hand grasping* (11), which in the case of the pellet is seldom successful (Figure 2, *Pellet 28*). The infant brings the pronated hand down on the pellet and then flexes the fingers into the palm as he attempts to close on the pellet. The action of the fingers so raises the palm from the table top that the pellet usually escapes on closure. When this grasping movement succeeds, the pellet lodges high on the heel of the palm. The inactive thumb usually curls into the palm or rests against the radial surface of the index finger. In the second type of closure, *palm grasping* (Figure 2, *Pellet 32*), the infant so brings the hand down on the table that the pellet lies near or under the radial portion of the palm, and then flexes the fingers and adducts the thumb. This action usually forces the pellet into the closed palm with the thumb firmly pressed against the radial edge of the index finger. In the *scissors grasp* (Figure 2, *Pellet 44*) the infant so places the pronated palm that the pellet lies under the index finger or on that portion of the table top which subtends the thumb–index-finger angle. Usually the three ulnar digits flex first, then the index finger flexes slowly, and finally the thumb adducts and pinches the pellet against the index or medius finger. This action, which resembles the movement of the hand in using a scissors, forces the pellet against the radial edge of the forefinger or into the radial palm against the medius.

In *vertical pincer grasping* (Figure 2, *Pellet 52*), the hand usually rests on the dorsum of the three flexed ulnar digits with the two radial digits extended so that the pellet lies under the index finger. The infant then adducts the thumb to a position under the index finger which curls vertically under its metacarpo-phalangeal joint so that the action brings the pellet between the volar surfaces of these digits. In *oblique pincer grasping* (Figure 2, *Pellet 60*), the infant rests the ulnar edge of the palm against the table top so that the thumb and index finger point medianward, as well as downward. He then adducts the extended thumb against the flexing index finger and pinches the pellet between the volar surfaces of the tips of the two digits (11). This form of closure is the typical grasp of adults.

No *superior oblique grasping,* in which the infant plucks the object without resting his hand on the table top, occurs, although we are certain that at 56 and 60 weeks voluntary control of arm movements has reached the point that the infant can so grasp the object if the occasion for such action arises (see p. 51).

Thumb opposition in grasping of small objects occurs late in the first year of life, because the negligible height of the objects requires that the infant hold his palm[8] so close to the table top that it not only interferes with, but actually prohibits, oppositional movement by the thumb.

---

[8] Infants in the first year usually place the hand on the table top beside the object, as they grasp it (13, p. 219).

Infants cannot, as a rule, maneuver their hands so that they can grasp the pellet until they are 28 weeks old. Table 2, *Pellet,* shows that from 28 weeks to 60 weeks improvement in prehension from hand grasping to oblique pincer grasping occurs slowly but steadily.

Successful grasping of the string starts at 28 weeks and improves gradually in precision up to 60 weeks. Figure 2, *String,* shows in order the following types of grasping: hand, palm, palm again, scissors, vertical pincer, and oblique pincer.

Theoretical Discussion

The eight objects elicit several varieties of grasps even within types. A given type of grasp has certain fundamental characteristics which distinguish it, such as the position of the object within the palm. On the other hand, a given type of grasp varies in detail in accordance with the form of the object. A forefinger (pincer) grasp on a pellet discloses that the digits are closely grouped, while only the three radial digits actively engage in gripping. This type of grasp on the cube shows that the digits are comfortably spaced. When this grasp is applied to large objects, such as balls, all digits spread widely and assist in gripping the objects (see Figures 1 and 2).

Classification of grasping in the case of the younger infants is at times difficult. Lack of voluntary control in reaching results in gross displacements of the hand upon the object so that absurdities in grasping appear. For example, these infants acquire a radial grasp when closure is clearly ulnar. They secure a precarious, cramped digital hold at an early age—a hold which has none of the true characteristics of a digital grip—merely because an unadjustable closure occurs before infants establish the correct relative position of hand and object. They grasp spoons, rods, and cups interdigitally and sometimes miss them altogether or brush them aside.

For the first 6 months, then, infants so close the hand on objects that the fingers force it against the palm of the hand. Up to 16 weeks, the point toward which the digital forces concentrate is the base of the palm midway of the thenar and hypothenar eminences. From 16 to 24 weeks, this point shifts distally toward mid-palm. From 24 to 32 weeks, as the radial fingers come into prehensory prominence, the digital forces converge upon the distal portion of the thenar eminence. From 32 to 40 weeks, the index finger which now firmly establishes itself as the leading digit in all manipulatory activity combines with the medius (the ring and little fingers cooperate on large objects) in pressing objects against the proximal thumb. From 40 to 60 weeks, infants grasp objects by pressure of the palmar pads of the finger tips against the distal volar pad of the thumb. Thus, in the development of grasp the position of the object within the hand shifts from proximal palm to distal volar thumb (Figure 2, *Position of Object in Palm,* and Figure 2, *Aspects of Hand Development,* DL).

Roughly, there are three outstanding stages in the development of grasping. The first stage, which covers the first 24 weeks of life, we characterize as the palming stage. All objects are forced into the palm mainly by the three ulnar digits with the thumb playing a very minor rôle. In the third stage, which begins definitely at 36 weeks and continues through life, digital grasping supersedes palming. Between these two stages we find the intermediate stage of grasping. It is the period of adjustment and refinement of crude, whole-hand gripping into adaptive, digital grasping. Many errors in grasping occur at this intermediate period because of failure in precise placement of one or more digits upon the object. Hence, many grasps which begin in superior manner end in inferior types of holding.

There are two developments in digital disposition which accompany progress in grasping. (1) Infants during the first half year automatically close the fingers (particularly the three ulnar digits)[9] upon objects with a firmness which is entirely out of proportion with the force which is actually required to hold them. By the end of the first year, infants make fine digital adjustments in grasping and hold objects by delicately balanced digital placement with just sufficient pressure for insurance against loss of the objects. (2) Digital flexion undergoes a gradual transformation as grasping improves. In early infancy all joints of the digits flex sharply to encircle the objects. As grasping develops and closure loses its vise-like force, the digits which are directly concerned in holding flex less and less at the phalangeal joints until at 60 weeks the hand flexes largely at the metacarpo-phalangeal joints, a type of flexion which brings the tips of the fingers in flat opposition to the distal pad of the thumb. In grasping a large object (such as a ball), where the hand experiences difficulty in gripping, flexion of all joints occurs at all ages and thus enhances "digging in" by the tips of the digits.

---

[9] Perhaps practice in prehensory activities accounts largely for the shift from palmar to digital grasping. The three ulnar digits function principally in palmar grasping because in flexion they close on mid-palm. First attempts in the utilization of the thumb and index finger in purely digital grasping fail because of lack of experience, but continued practice in grasping by the latter digits results finally in skilled digital manipulation. While the writer believes that experience in prehensory activities aids in refinement of reaching and grasping movements, the initiation of participation by the radial digits in all prehensory activities at 28 weeks and the exploratory movements by the index finger (see pp. 50, 57) hint at a neurological basis for these changes in motor reactions—the maturation of the sensory nervous system and of the neuro-musculature of those parts of the arm and hand which concern themselves with prehension.

A study of certain aspects of the development of the nervous system and musculature in early infancy reveals several interesting facts which may in part explain why grasping during the first year of life changes from tenacious palmar gripping to adaptive finger tip prehension.

In human beings all neurons or neuroblasts which adults possess are present at birth (2). The time of beginning of functioning by individual neurons, is in most cases, however, not well known (7, pp. 222 ff.). Neurological evidence (pp. 49-50, 57-58) points to an imperfectly developed condition of peripheral nerves and muscles for several weeks after birth. Under this condition an infant does not for some weeks, possibly months, enjoy, as he does later, the full benefit of sensory experiences from his exteroceptive and proprioceptive systems. Neither does he enjoy to any extent full use of his motor mechanisms, as neither the effector nerves to the muscles nor the musculature itself is functionally well developed. Sensory data which do reach the cortex do not aid him in bodily adjustments to external situations, because, as we shall see, even if the peripheral effector neurons and muscles were fully developed, the effector pathways from the cerebral cortex are not functionally mature.

The state of development of the nervous system during the first year of life gives some indication of the manner in which voluntary arm and hand movements replace reflex reactions. Although we know little about the function of the myelin sheath in medullated nerve fibers, the general belief is that the presence of the sheath signifies the readiness of the neuron for the transmission of impulses (2, 16, 35).

The condition of myelination of nerve tracts of the brain and spinal cord shows that neither the cerebral cortex nor the pyramidal tracts[10] (the great effector pathways for voluntary movements) is developed to the extent that they can function in control of motor reactions until the second or third month of post-natal life (8). Then, it is likely that several weeks pass before cortical control advances to the stage when its effects on such movements as reaching and grasping are apparent to the observer. This investigation indicates that reflex groping and grasping movements function strongly for the first 16 weeks of life, after which time these reactions gradually lose their sporadic, automatic character. At 52 weeks reaching and grasping are completely under voluntary control. Watson (37) states that the grasping reflex disappears as early as the 80th day in one infant and in other infants functions for a period greatly exceeding 5 months. Adie and Critchley set 4 or 5 months as the date at which reflex grasping shows signs of weakening.[11]

---

[10] A second effector pathway to the voluntarily controlled muscles, concerning which we have no exact knowledge, is the extra-pyramidal path. It appears that the cerebral cortex connects indirectly by way of the thalamus with the striate body from which axons pass down the striatorubral tracts to the red nucleus and then through the rubro-spinal tracts and peripheral nerves (2, pp. 134, 440).

[11] Some neurologists believe that the thalamo-striate brain functions in the production of primitive, automatic, associated muscular activities of certain higher animals, and has its human counterpart in the thalamo-pallidum.

According to Foerster (9), Kleist (21), Schuster and Pinéas (30), the thalamus and globus pallidus form the nervous center for the regulation of automatic reactions during the first 5 months of human infancy. Later, as the cerebral cortex and striatum develop, these reactions undergo gradual modifications until finally voluntarily controlled motor reactions replace automatic movements.

As far as we know, at present no experimental evidence of the influence of the spinal ganglia upon the spinal centers exists, although, as Berry (2, p. 439) says, "the thalamus appears to give off neurons which pass to the globus pallidus, thus retaining the primitive thalamo-striate connections."

Adie and Critchley (1) ridicule the theory of "thalamo-pallidal" control and point out that perfection of operation of very highly complex automatic reactions which occur in mid-brain animals (23) removes the necessity for the assumption of a thalamo-pallidal brain in early infancy. In addition, Adie and Critchley show that in adults who have lost voluntary control of arm and hand movements through tumors or wounds in the brain and therefore grasp in the manner of very young infants no instance of reflex grasping occurs in which lesions are confined entirely to the basal ganglia (of which the striatum is a part) but that in every instance of reflex grasping lesions are found in the frontal cerebral cortex even when no lesions in the basal ganglia are present (32).

Now Pavlov (25) shows that the cerebral cortex is the anatomical basis for the establishment of acquired reflexes. Hence, voluntarily controlled (conditioned) movements, such as reaching and grasping "must be subserved" by the cortex (3).

If muscular development proceeds peripheralward (6), cerebral-cerebellar influence in prehension should manifest itself first in movements of shoulder and upper arm and gradually extend toward the digits (see next paragraph). The data indicate that crude voluntary shoulder and elbow movements appear before voluntary movements of other parts of the arm in reaching and grasping (see p. 35). For example, at 16 weeks infants swing their arms at the shoulder laterally in abductive-adductive manner, often in rotary fashion, and make little or no forward progress toward the object of their regard. When they are 20 weeks old they bring the arms forward and downward in patting, pawing, or clawing movements in which lateral sweeps continue to function to a great degree. The infants, instead of actually grasping an object which the examiner places before them, first corral it by inward circuitous, sweeping movements of the forearm and hand, and then, when they have pressed it against some portion of the body, they attempt to close on the object. Indeed, at 16 to 20 weeks, when the upper arm under slight voluntary control is already moving the hand toward objects, the types of movements which the hand manifests are reflex grasping and microkinetic reactions (36).

Likewise the data indicate that in order of appearance crude palmar grasping precedes digital grasping. Heptner (15) compares the arms of a newborn baby with the right arm of a 3-year-old boy and the arms of two men and concludes that the forearm of the newborn boy, as contrasted with the upper arm, is in a lower state of development with respect to both musculature and nerves. The ratio of growth of the peripheral nerves of the arm of the 3-year-old boy to the newborn boy is 4 to 1. These facts indicate that development proceeds peripheralward and that peripheral muscles and nerves are not fully developed at birth. In addition, Westphal (38) and MacCallum (7, pp. 350 f.) both show that, microscopically, the individual muscular fibers of newborn infants have about one-third the diameter of similar fibers of 1-year-old infants. In addition, the fibers at birth are round, while in adulthood they are polygonal (38, p. 74). Some of them become polygonal at 3 weeks, while others remain round even after 1 year (38). Infants' muscles, as compared with adult muscles, are paler in color, contain more water, and are more delicate (7, p. 201 f.).

Further investigations by C. Westphal (39) and A. Westphal (38) show that muscular response to electric stimulation in children varying in age from 1 hour to 8 years is low for the first few weeks of life and that muscular contraction occurs at a slower rate in these infants than in older infants. Also, injections of curare react slower in production of paralysis of motor nerve endings of muscles of newborn animals than of adult animals (26).

The low state of excitability, the latent period of response, and the microscopic structure of muscles of infants indicate that for at least the first few weeks these motor mechanisms of the body are not fully developed (see p. 57).

Appreciation of Form, Size, and Weight

Under the condition of this experiment grasping depends upon the infant's capacity in perception of relationships in visual space and his ability in coordination of movements of the muscles of trunk, arm, and hand in reaching for and grasping the visually perceived object. Proper functioning of the hand in reaching and grasping requires constant central correlation of exteroceptive impulses from the eye and proprioceptive impulses from the muscles, tendons, and joints as they undergo changes of extension, contraction, flexion, or tension (10, 14, 17), so that appropriate adjustments of the moving parts of trunk and arm may occur smoothly (36). Precise digital grasping takes place only when the infant can so distribute the individual digital pressures against the object that they establish a delicate balance between the opposing forces of gripping posture. This statement explains why grasping and manipulation of objects by the finger tips—the highest types of prehension—take place only near the end of the first year of life.

Infants of less than 24 weeks of age give not the slightest evidence of appreciation of form, size, or weight of objects. These infants hold objects in a persistent vise-like grip which relaxes only through sudden shock or change of activity. We have seen that the younger infants do not adapt the force of their grip in accordance with the weight of the object. They either cannot adjust the hand properly or they fail in adequate perception of the size, mass, form, and surface characteristics of the object. This type of grasping contrasts strangely with the delicate precise digital grasping by older infants (40 to 60 weeks) in which exact placement of the digits and economy in gripping pressure indicate an appreciation of the physical qualities of the object. Adaptation of pressure to mass of objects in grasping functions at 32 weeks or later, when objects are held by the digits.

The disposition of the digits in reaching is a good indication of infant appreciation of size and form of objects. Infants up to 6 months reach with pronated hands, the thumb extending passively in line with the fingers. The fingers usually remain extended until the palm contacts with the object and the thumb remains quite inactive even in grasping. The object sometimes slips from the hand because of misjudgment of direction or distance in placement. Re-adjustment of the hand for improvement of grasp never follows poor hand placement because the infant cannot voluntarily relax his grip. Infants of 40 to 60 weeks of age in reaching gradually align the forearm and radial digits with the object and adjust the digits to its size and form. For large objects, as the ball, infants maintain a wide open gap between the thumb and other digits. For small objects, as a string or pellet, infants close the thumb-forefinger gap so that only slight additional flexion of these digits is required for closure.

The disposition of the digits in reaching and grasping then shows appreciation of the object's form and size. Plucking the object from the table top, economy of digital pressure in holding, ease of transfer from hand to hand, repeated releasing and grasping, and regarding the hand as it slowly releases an object perhaps indicate appreciation of the mass of the object. Now reaching and grasping of the types which these older infants display occur only when the proprioceptive nervous system of the hand and forearm functions in a fairly high degree of perfection. In addition to the fact that the muscles of the arm and hand for the first weeks of life are in a low state of development (see p. 50), certain researches (2, 4, 7, 24, 27, 38) indicate that incompleted development of both the peripheral nerves and the higher nervous centers for several weeks of life accounts for the retarded functioning of the proprioceptive system in reaching and grasping. The exteroceptive system of the hand perhaps aids in the formation of these judgments of external objects but the principal function of this system is the perception of their surface characteristics, such as roughness, smoothness, sharpness, hardness, etc.

The data show that at the age of 28 weeks infants for the first time exhibit reactions which indicate appreciation of physical qualities of external objects. At 52 weeks all infants indicate by their manner of reaching, grasping, and manipulation that they make judgments of size, form, and mass.

Muscular Action in Grasping

The evidence from this study and from an earlier investigation (13) indicates a tardy ripening of activities by thumb and index finger and their gradual usurpation during the second half year of life of prehensory functions which for the first 6 months are exercised principally by the three ulnar digits.

The reflex grasp of early infancy suggests that the exteroceptive and effector nervous mechanisms which function in bringing into action the musculature of the ulnar hand respond adequately under control of a lower center in the central nervous system, but that the proprioceptive system to the hand, if it functions at all, does not aid in voluntary adjustments of hand or digits. In addition, the almost total absence of movement by the thumb and the occasional closure reactions by the forefinger suggest the possibility of fully developed peripheral motor nerves to the tendons of the ulnar digits and immature effector pathways to the index finger and thumb.

The median nerve (40, p. 293) supplies the entire flexor digitorum sublimus muscle and the radial portions of the flexor profundus and lumbricals (see p. 54), while the ulnar nerve supplies the ulnar halves of the flexor profundus and lumbricals (40, p. 284). The motorial result of paralysis of the median nerve, then, is loss of power of grasp by the three radial digits, especially by the index which assumes a posture of partial extension (20, 34, 40). The thumb lies extended and adheres closely to the index finger. This posture (20, p. 1424) of the hand in which the two ulnar digits retain practically full power of flexion, an action in which the medius joins, resembles somewhat the gripping posture of early infancy, which is illustrated in the ulnar grasp (see Figure 1, *Bell 24*). Paralysis of the median nerve, however, does not prevent partial flexion by the index and medius, because the two ulnar digits in the act of flexion draw along with them these two radial fingers (40, pp. 294 f.).

On the other hand, it is likely that reflex grasping does not involve the index finger and thumb to the degree that such grasping concerns the three ulnar digits, because, inasmuch as the two radial digits are highly specialized manipulatory organs, they are subject principally to control by the cerebral cortex. Perhaps by means of special division and arrangement of the musculature of the hand the two specialized radial digits have succeeded in partial emancipation from reflex muscular control, and this freedom is reflected in their lack of active response in early grasping and in their adaptation in skilled movements which involve cortical control.

A review of the anatomy of the hand shows that grasping in early infancy is brought about by the concerted action of three muscles; the flexor digitorum profundus, which gives off branches to all digits;

the flexor digitorum sublimis, an extrinsic muscle which converges upon tendons of the four fingers (there is no flexor sublimis to the thumb), and the lumbricals, four in number, which originate in the tendons of the flexor profundus (40, pp. 172 ff.). The four lumbricals extend one into each finger and in contraction flex the metacarpo-phalangeal joints (40, p. 214). Contraction of the flexor sublimis flexes the middle phalanx on the proximal phalanx of each finger. The flexor digitorum profundus in action flexes the terminal joints of all digits. The latter muscle consists of three distinct parts: an ulnar division which gives off three tendons which extend into the three ulnar digits, a second division which passes into the index finger, and a third division which is inserted into the thumb (40, pp. 175-178). Now two of these muscles (the lumbricals and the flexor sublimis) entirely ignore the thumb, while the third muscle (the flexor profundus) sets off the index finger and thumb from the three ulnar digits by independent tendons. Thus, we can see that in reflex grasping stimulation of the palm of an infant may cause flexion of all joints of the three ulnar fingers and only partial flexion of the index finger without movement of the thumb. Early grasping shows that (1) the three ulnar digits flex strongly at all three joints, (2) the forefinger flexes least at the terminal joint, and (3) the thumb, in general, flexes not at all. It is likely then that in early grasping the three principal flexor muscles of the ulnar digits usually function at full strength, while the two divisions of the flexor digitorum profundus to the thumb and index finger function irregularly, with respect to amount and force of flexion.[12]

The writer inclines toward the belief that special division and arrangement of the muscles of the hand, rather than retarded development of the median nerve, account for the peculiarities of early grasping, because, although the index finger may or may not accompany the three ulnar digits in closure on objects when it does close, it often flexes with considerable force. This force is undoubtedly inherent in this digit, because the examiner may forcibly remove the ulnar digits from their gripping posture without material alteration of the grasping force by the index finger.

---

[12] The thumb takes very little part in early grasping, while the index finger often reacts independently of the three ulnar digits. Sometimes it flexes tightly with them (Figure 1, *Cube 12*); sometimes it only partially contracts (Figure 1, *Cube 24*), and at other times it remains almost fully extended (Figure 1, *Bell 24*). One offers in partial explanation of this fact that that part of the extensor digitorum communis, the principal extrinsic muscle of the fingers, which flexes the metacarpo-phalangeal joint of the index finger is most clearly separated from the remainder of its muscle mass and perhaps functions more or less independently (40, p. 179).

A finger dynamometer which registers the individual pressures of the digits in grasping should reveal the extent to which each digit aids in holding and the changes in pressure by each finger during the first six months of life.

The skin-folds or crease-lines of the palmar surface of the hand furnish some indication of the manner in which the digits cooperate in closure. The three principal lines (40, pp. 65-75) are: the distal transverse crease which starts at the ulnar edge of the palm and after running radialward for some distance curves abruptly and ends at the lower palm at the cleft between the index and medius fingers; the proximal transverse crease which starts at the radial edge of the palm and crosses it obliquely; and the radial longitudinal line which starts near the palmar wrist and encircles the thenar eminence, or ball of the thumb. The distal transverse crease is most marked toward the ulnar palm and is formed by the flexion of the three ulnar digits. The proximal transverse crease is most prominent at the radial palm and is caused mainly by flexion of the index and medius fingers. The radial longitudinal crease is the result of adduction of the thumb. The skin, then, as well as the muscles, indicates that the three ulnar digits cooperate in certain types of closure, probably in palmar gripping, and that the index and medius fingers combine in another type of closure, very likely in skilled movements which involve the digital tips. The thumb is so hinged that it may easily adjust by adductive movements in opposition to any one finger or to any combination of fingers.

Tactual Sensibility and Its Relation to Grasping

A very material transformation in the character of manipulation of objects occurs at about 28 weeks. Crude palming of objects surrenders to digital activity. Mere holding of objects gives way to index finger activity in "feeling" and thumb cooperation in handling of objects. This new type of prehensory activity becomes marked at 36 weeks and manifests itself continually during the latter part of the first year of life.

In early infancy the infant does not distinguish between digits. In reaching he swings the hand paw-like at the object and in grasping he gets the major portion of the palmar surface of the hand against the object by projection of the hand over its top surface. He neglects the thumb which may or may not contact with the object. In fact, for the first 24 weeks of life, if we except crude grasping, no prehensory activities occur which suggest that infants tactually perceive physical qualities of external objects. After 28 weeks thumb and index finger activity in manipulation of objects appear. The infant deliberately aims the hand so that its forward thrust carries the volar tip of the thumb against the near surface of the object and permits forefinger placement against the distant surface.

Perhaps the infant fingers, pushes, tips, and rolls objects about on the table top by the palmar tip of the index finger and indulges in other digital activities merely for exploitation of the object. Perhaps, however, he is enjoying new experiences in these manipulatory activities. With his limited experience, it is likely that objects which vary so greatly in size, form, weight, and surface qualities may bring about prehensory movements which permit contact with the objects by those portions of the hand which in adults are richly endowed with tactual receptor organs. The writer believes that at about 28 weeks tactual sense perception functions for the first time in appreciation of exterior objects. Among other activities which appear in support of this view are: pointing by the index finger in reaching; ulnar flexion of the wrist so that the volar pads of thumb and index finger make first contact with objects in grasping; grasping by digital tips; repeated application and removal of finger tips; poking with them; picking up and putting down objects immediately; rolling the object between the three radial digital tips in holding; quickly repeated transfer of objects from hand to hand; and rubbing the end of the index finger of one hand against the object with close inspection as he grips it by the other hand; decrease in mouthing of objects; and the increasing prominence of index-finger activity in all types of exploratory manipulation. With respect to mouthing, one finds that up to 32 weeks infants usually carry objects to the lips. From 32 to 52 weeks this hand-to-mouth activity gradually disappears. The highly sensitive lips (29; 27, pp. 76-90) undoubtedly influence the early hand-to-mouth activity, because the lips aid not only in acquisition of food by sucking but also assist in acquisition of sensory data. It is likely that, as sensitivity of the finger tips increases, the hand-to-mouth reactions weaken because of the failure of the lips in the accumulation of sensory impressions which are not obtainable by the digits. In passing, if the tip of the index finger, more than any other portion of the hand (29), is most sensitive to cutaneous stimulation, why is this digit in early prehension the least utilized finger of the hand?

Investigations on the state of development of peripheral nerves in the arm indicate that tactual sensibility of the digits of very young infants is low (20, 27). Remak (7, p. 227), Bakitko (7, p. 228), and Westphal (38, pp. 81-93) state that peripheral nerves possess at birth only a few myelinated fibers, while Preyer (27, pp. 83-84) remarks that conductivity by the nerves to the skin increases gradually after birth.

Westphal (38, p. 21) tests the peripheral nerves of children varying from 1 hour to 8 years of age and finds that infants up to 3 weeks of age can tolerate easily an electric current of an intensity which adults cannot endure. Among the nerves examined are the ulnar, radial, and median, the three nerves which contribute to cutaneous, muscular, and articular sensibility in the forearm and hand.[13] Westphal explains that this low excitability is due to incomplete development of the higher centers, and perhaps to the nature of the anatomical structure of the peripheral nerves in which the development of the myelin sheath is slow, gradual, and incomplete for several weeks after birth (38, pp. 72-74). Excitability, he says, does not reach its maximum until infants are over 6 weeks of age. Soltmann (31) claims that excitability in sensory nerves of rabbits and puppies increases until they are about 12 weeks old. Other researches (7, pp. 228 f., 385 f., 565-568) also reveal a higher excitability in nerves with increase in age, and Holmes (18) in an investigation of 380 children, ranging in age from birth to 13 years, finds that nerves attain adult value in excitability only when children have reached an age of 5 or 6 years.

Tactual stimulation of the palm of the newborn infant usually results in instantaneous reflex grasping. If peripheral nerves to the cutaneous surface of the palm or to its sub-cutaneous tissues are at this time in a state of incomplete development, we may well ask what kind of tactual sensibility excites grasping? A number of studies

---

[13] While variations in the peripheral ramifications of the nerves of the hand occur from individual to individual, branches of the ulnar nerve supply cutaneous sensibility to the entire palmar surface of the little finger and to the ulnar half of the ring finger. Branches of the median nerve supply cutaneous sensibility to the palmar surfaces of the three radial digits and to the radial half of the volar surface of the ring finger (14, 20, 33, 34).

(4; 27; 7, pp. 233 ff.) on cutaneous sensibility at birth indicates that the lips and nostrils are most sensitive to touch and that the tongue, the palm, and the sole of the foot also rank high in this respect. Infants apparently respond readily to thermal stimuli but the threshold for pain, however, is high. In view of the scanty information and the disagreement in results (2, p. 120; 7, p. 386), investigations in the field of cutaneous sensitivity in early infancy and childhood are in order.

At birth myelination in the cerebral cortex is completed only in those areas (primary sense zones and their marginal zones) which provide for the "receipt" and "storage" of receptor impulses (2, pp. 393 ff.; 28, pp. 289-295). Myelination within the remaining areas of the cortex (frontal, temporal, and occipital zones, generally known as the association areas) occurs sometime after birth (2, 8, 28).

If we combine the facts of peripheral and central development of the nervous system, it appears that sensibility of very young infants differs from sensibility of adults. We do not know whether in order of appearance one kind of general sensibility precedes another (i.e., muscular sensibility precedes tactual, or vice versa) or one kind of tactual sensibility precedes another. However, the indications are that appreciation of objects by tactual perception undergoes slow and gradual growth. The data (13) show that evidences of such appreciation by way of manipulatory exploitation and exploration of objects appear at 28 weeks and increase in number and variety up to 52 weeks.

Voluntary release of objects makes its appearance between the ages of 36 and 48 weeks, although one finds an instance of release at 32 weeks. The development of release follows a very natural course. At first infants merely hold objects, and do not release them. For example, an infant of 16 to 24 weeks of age who secures a precarious hold on an object cannot release the object and attempt a more favorable hold. He grips the object until it slips away or his attention drains to another object. An infant of 24 weeks who misjudges the distance in reaching and places his wrist and heel of the palm on the object, drags it about on the table in repeated efforts at closure on the object. At 24 weeks also, with both hands holding the object, one hand may free itself from the object and after some time may return to it. In the next stage, about 28 weeks, one hand pulls the object forcibly from the other hand. One infant of 28 weeks, in accomplishing this exchange, tugs with such force that he temporarily loses his equilibrium. At 32 and 36 weeks, transfer of the object from hand to hand occurs quite readily. At 40 and 44 weeks, the infant releases objects against some resisting surface such as the table top. At last, 44 to 56 weeks, he succeeds in voluntarily dropping objects freely without external resistance, and finally, 52 to 60 weeks or later, throws them.

The change in type of grasp from the crude, tenacious palmar grip of 16 to 20 weeks to the light, firm, balanced digital grasp of 52 weeks which lends itself so readily to manipulatory movements is of importance because this remarkable shift of location in hand activity (ulnar palm to radial digital tips) indicates that greatest progress in prehensory development occurs before infants reach the age of one year. The transformation in reaching which occurs during this period of development lends substantiation to the above statement (see p. 35). Figure 2, *Aspects of Hand Development, 3 Year and Adult,* presents illustrations of grasping by a 3-year-old child and an adult. These illustrations show that these subjects reach, grasp, and hold cubes quite in the manner of infants of 60 weeks. The 3-year-old child and the adult exhibit the straight approach in reaching, the superior-forefinger grip in grasping, and nicely balanced digital placement in holding. When objects such as spoons and cups are presented to the older subjects, greater length and strength of digits, knowledge and practice in the use of these articles give the older subjects certain advantages in grasping and manipulation over the 60-weeks infants. The older subjects can hold the cup upright so that nothing spills from it. The 60-weeks infants are not likely to hold it erect. The older subjects grip the spoon at the handle between the thumb and two fingers. The 60-weeks infants use the same type of grip, but grasp the bowl, neck, or handle. In directness and skill of approach, in neatness and dispatch of grasp, however, very little improvement occurs after 60 weeks.

Advantages of Digital Grasping

Practice, of course, functions in perfection of prehension. However, in grasping during the first year of life the urge or motive which underlies the progressive movement of the object from palm to radial digital tips is most likely facilitation of manipulation and of tactual perception. The digits from a mechanical standpoint form a system of levers of the third class. They function individually and in combinations in exploitation of exterior objects. The metacarpo-phalangeal joints of the hand serve as the fulcrum, the flexor muscles and tendons of the digits are the acting force, and the digits and object form the resisting force. Obviously, the more distal on the digits one holds an object, the greater must be the leverage with which the digits grip the object. It is equally true, however, that remote holding aids in manipulation, because this type of gripping, as compared with palmar gripping, involves less cutaneous contact with the object, which in turn permits freedom of movement of the object by the digits and greatly facilitates transfer, release, throwing and exploratory activities which require frequent re-adjustments by the thumb and fingers. Holding by digital tips has two other distinct advantages over palmar gripping. In the first place, holding by the digits alone necessitates fine control over the entire musculature of the hand, a condition which depends largely upon perfection of function of the proprioceptive system whose receptor organs are in muscles, tendons, and joints of the hand. This full play of the proprioceptors of the hand and digits not only enhances delicacy of pressure and preciseness of placement by the individual fingers in grasping and dexterity in manipulation, but aids in the formation of perceptions of size, form, and mass of objects. In the second place, digital grasping brings objects into direct contact with those portions of the volar hand which are most sensitive in the perception of surface qualities of objects.

The type of grasp which infants apply to an object is then an indication of their ability in manipulation of the object. The palm grasp of early infancy is merely a *holding* grip; the digital grasp of the 1-year infant is a *feeling* grip. The older infant resorts to palmar grasping only when reaching difficulties interfere with prompt acquisition of the object, or rough play, such as banging the object against the table top, follows grasping.

Early grasping is immediate, unadjustable, forceful, and tenacious. At one year grasping is deliberate, adaptable with respect to size, form, or weight of the object and only as strong and tenacious as the occasion requires. All skilled prehensory movements involve principally the use of the thumb and index finger.

Conclusions

1. The index to development of prehension is the position within the hand toward which the digital forces that function in the act of grasping concentrate. If, then, we plot on the hand the six

points which successively represent the focus of these individual forces in each of the progressive stages in the development of grasping, the line joining these points has its origin in the upper portion of the palm midway of the thenar and hypothenar eminences. The line extends first to mid-palm, then to the lower end of the median of the thenar eminence, and finally out to a point on the tip of the thumb.

2. The type of grasp which an infant uses depends in part upon the length of the digits, in part on the maturation of the neuromusculature of the hand, in part on the development of tactual perception by the digits and, finally, on the infant's ability in the coordination of both coarse and fine arm and hand adjustments in reaching with visual-spatial perception of the object which is to be prehended.

3. Infants manifest appreciation of size, weight, form, and surface characteristics of prehended objects after the first 6 months of life. Voluntary control over the digits in grasping appears between 16 and 24 weeks. Investigations on the condition of myelination in the central nervous system and on the low state of development and excitability of the peripheral nerves and muscles lend corroboration to the above statement.

4. Thumb opposition in grasping begins to function as the grasping reflex disappears. At this time, also, sharp flexion at the phalangeal joints of the hand and intense gripping gradually diminish, so that, by the time the finger tips replace the palm in grasping, the infant adjusts the force of the grip to the mass of the object. At 60 weeks the fingers which close on all except large objects are pretty well extended and flex largely at the metacarpo-phalangeal joints.

5. Ulnar flexion of the wrist and thumb opposition in grasping develop simultaneously.

6. Early grasping is enveloping and forceful. Grasping at one year is precise and delicate.

7. In action the hand of the very young infant resembles a paw. Differentiation of digital function does not occur until after 20 weeks of life.

8. The ulnar digits function strongly in reflex grasping. The thumb and index finger function principally in skilled prehensory movements.

9. Voluntary release occurs only at the end of the first year.

10. In accuracy of reaching and in precision of grasping, infants of 60 weeks rank on almost equal terms with adults in prehension of objects of regular form and of average size.

REFERENCES

1. ADIE, W. J., & CRITCHLEY, M. Forced grasping and groping. *Brain,* 1927, **50**, 166-170.
2. BERRY, R. J. A. Brain and mind. New York: Macmillan, 1928. Pp. 608 (see esp. 12-13, 107-108, 120, 134-138, 274, 383-385, 393-397, 434-440, 468-469).
3. BIANCHI, L. The mechanism of the brain and the function of the frontal lobes. (Trans. by J. H. MacDonald, E. Livingstone, & S. Livingstone.) Edinburgh: 1922.
4. CANESTRINI, S. Ueber das Sinnesleben des Neugeborenen. Berlin: Springer, 1913. Pp. lv+104.
5. CASTNER, B. M. The development of fine prehension in infancy. *Genet. Psychol. Monog.,* 1932 (in press).
6. COGHILL, G. E. Anatomy and the problem of behavior. (Lectures delivered at University College, London.) New York: Macmillan; Cambridge: Cambridge Univ. Press, 1929. Pp. xii+113.
7. FELDMAN, W. M. Ante-natal and post-natal child physiology. New York: Longmans, Green, 1920. Pp. xxvii+694 (see esp. 201-204, 213-229, 233-235, 350-356, 385-386, 556-558, 564-568).
8. FLECHSIG, P. Anatomie des menschlichen Gehirns und Rückenmarks auf myelogenetischer Grundlage. (I. Band.) Leipzig: Thieme, 1920.
9. FOERSTER, O. Zur Analyse und Pathophysiologie der striären Bewegungstörungen. *Zsch. f. d. ges. Neur. u. Psychiat.,* 1921, 56-73.
10. FULTON, J. F. Muscular contraction and the reflex control of movement. Baltimore, Md.: Williams & Wilkins, 1926. Pp. xv+644.
11. GESELL, A. L. Infancy and human growth. New York: Macmillan, 1928. Pp. 418.
12. ————. The guidance of mental growth in infant and child. New York: Macmillan, 1930. Pp. xi+322.
13. HALVERSON, H. M. An experimental study of prehension in infants by means of systematic cinema records. *Genet. Psychol. Monog.,* 1931, **10**, 107-286.
14. HEAD, H. Studies in neurology. (2 vols.) London: Frowde & Hodder, 1920. Pp. 338; 537 (see esp. Vol. 1, 55-98, 255; Vol. 2, 588-589, 604, 661, 667, 668, 691-694).
15. HEPTNER, W. Das Massenwachstum der peripheren Nerven und anderen Gewebebestandtheile der Extremetäten nach der Geburt. *Arch. f. Anat. u. Physiol., Anat. Abt.,* 1915, 277-295.
16. HERRICK, C. J. An introduction to neurology. (4th ed.) Philadelphia: Saunders, 1927. Pp. 406.
17. HOLMES, G. Disorders of sensation produced by cortical lesions. *Brain,* 1927, **50**, 426.
18. HOLMES, J. B. The reliability of the electrical diagnoses of tetany. *Amer. J. Dis. Child.,* 1916, **12**, 1-29.
19. HOWELL, W. H. A textbook of physiology. (7th ed.) Philadelphia: Saunders, 1918.
20. [ED.] JACKSON, C. M. Human anatomy, by Morris. Philadelphia: Blakiston, 1921. (See esp. 796-798, 1018-1026, 1424-1426.)

21. KLEIST, K. Die psychomotorischen Störungen und ihr Verhältnis zu den Motilitätsstörungen bei Erkrankungen der Stammganglien. *Monatssch. f. Psychiat. u. Neur.*, 1922, **52**, 263-282.

22. LEWIS, W. H. Development of the arm in man. *Amer. J. Anat.*, 1901-1902, **1**, 181.

23. MAGNUS, R. Some results of studies in the physiology of posture. (Cameron prize lecture.) *Lancet*, 1926, **211**, 531-536, 585-588.

24. OTTOLENGHI, S. La sensibilité et l'âge. *Arch. ital. de biol.*, 1895, **24**, 148.

25. PAVLOV, I. P. Conditioned reflexes. An investigation of the physiological activity of the cerebral cortex. (Trans. by G. V. Anrep.) London: Oxford Univ. Press, 1927. Pp. xvi+430.

26. PREYER, W. Special Physiologie des Embryo. Leipzig: Grieben, 1885.

27. ————. Die Seele des Kindes. (3. Aufl.) Leipzig: Grieben, 1890. Pp. 504 (see esp. 76-90, 249-250, 472-473).

28. RANSON, S. W. The anatomy of the nervous system. (3rd ed.) Philadelphia: Saunders, 1927. Pp. 425 (see esp. 289-291, 295).

29. SCHÄFER, E. A. Textbook of physiology. Vol. 2. Edinburgh & London: Pentland, 1900.

30. SCHUSTER, —, & PINÉAS, H. Weitere Beobachtungen über Zwangsgreifen und Nachgreifen und deren Beziehungen zu ähnlichen Bewegungsstörungen. *Dtsch. Zsch. f. Neur.*, 1926, 50-55, 91.

31. SOLTMANN, O. Ueber die Erregbarkeit der sensiblen Nerven der Neugebornen. *Jahrb. f. Kinderhk.*, 1879, **14**, 308-315.

32. STIEFLER, G. Anatomischer Befund in 2 Fällen von Steigerung des Grundgelenkreflexes mit gleichzeitigem Zwangsgreifen bei Stirnhirnbalkentumor. *Dtsch. Zsch. f. Neur.*, 1926, **89**, 161-178.

33. STOPFORD, J. S. B. The anatomy of so-called deep sensibility. *J. Anat.*, 1918-1919, **53**, 14-25.

34. ————. A new conception of the elements of sensation. *Brain*, 1922, **45**, 385-414.

35. TILNEY, F., & RILEY, H. A. The form and functions of the central nervous system. New York: Hoeber, 1921.

36. WARNER, F. Muscular movements in man and their evolution in the infant. *J. Ment. Sci.*, 1889, **35**, 33-.

37. WATSON, J. B. Behaviorism. (Rev. ed.) New York: Norton, 1930. Pp. 319.

38. WESTPHAL, A. Die electrischen Erregbarkeitsverhältnisse des peripherischen Nervensystems des Menschen in jugendlichem Zustand und ihre Beziehungen zu dem anatomischen Bau desselben. *Arch. f. Psychiat. u. Nervenhk.*, 1894, **26**, 5-25, 72-75, 81-93.

39. WESTPHAL, C. Gesammelte Abhandlungen, 1892, II, No. 56, 833-834.

40. WOOD-JONES, F. The principles of anatomy as seen in the hand. London: Churchill, 1920. (See esp. 65-79, 172-179, 214, 284, 293-295.)

Yale University
New Haven, Connecticut

AUTRE ÉTUDE DE LA PRÉHENSION

(Résumé)

L'auteur emploie la méthode cinématographique dans une enquête pendant quatre ans de la préhension chez 7 enfants qui sont examinés régulièrement à des intervalles de 4 semaines en commençant à l'âge de 12 semaines et en terminant à l'âge de 60 semaines. Les objets saisis par les enfants varient beaucoup en grandeur, en masse, et en forme, et exigent une grande variété de types de préhension. Les conclusions principales sont:

1. L'indice de l'étape du développement de la préhension c'est la position dans la main vers laquelle se concentrent les forces opposées qui fonctionnent dans l'action de saisir.
2. Le type de préhension employé par un enfant dépend en partie de la longueur des doigts, en partie de la maturation de la névro-musculature de la main, en partie du développement de la perception tactile par les doigts, et enfin de l'habileté de l'enfant à coördiner des ajustements délicats et non délicats des bras et des mains quand il les étend avec la perception visuelle de l'objet à saisir.
3. L'opposition du pouce dans la préhension commence à fonctionner comme le réflexe de préhension disparaît. A ce temps la brusque flexion des doigts et l'intensité de la préhension deviennent de moins en moins grandes, de sorte que l'enfant adapte la force de la préhension à la masse de l'objet quand les bouts des doigts remplacent la paume dans la préhension.
4. La plus grand développement de la préhension chez l'homme a lieu avant l'âge de 60 semaines.

HALVERSON

EINE WEITERE UNTERSUCHUNG DES GREIFENS

(Referat)

Unter Gebrauch der kinematographischen Methode untersuchte der Verfasser über vier Jahre das Greifen (grasping) bei sieben Kindern, die regelmässig alle vier Wochen untersucht wurden, von der zwölften Lebenswoche bis einschliesslich zur 60sten. Die Gegenstände die von den Kleinkindern erfasst werden sind in Bezug auf Grösse, Masse und Form sehr verschieden, und nehmen sehr verschiedene Formen des Fassens in Anspruch. Die wesentlichsten Befunde sind folgende:

1. Der Anzeiger (Index) des Entwicklungsgrades des Greifens ist die Lage innerhalb der Hand gegen welche die entgegengesetzten Kräfte die im Fassen tätig sind sich konzentrieren.
2. Die Art des Fassens die ein Kleinkind gebraucht ist abhängig teilweise von der Länge der Finger, teilweise von der Reife des Nerven-Muskelsystems der Hand, teilweise von der Entwicklung der Tastempfindung (tactual perception) durch die Finger, und endlich von der Fähigkeit des Kleinkindes sowohl grobewie feine Anpassungen des Armes und der Hand bei dem Langen (reaching) unter visueller Wahrnehmung des zu erfassenden Gegenstandes zu koordinieren.
3. Die Opponierung des Daumend bei dem Greifen beginnt seine Wirkung im Laufe des allmähligen Verschwindens des Greifreflexes (grasping reflex). Zu dieser Zeit vermindern sich allmählig die starke Biegung der Finger und die Intensität des Ergreifens. Zur Zeit, also, wo die Fingerspitzen die Handfläche bei dem Greifen ersetzt haben passt das Kleinkind die Stärke des Ergreifens der Masse des Gegenstandes an.
4. Die stärkste Entwicklung des Greifens beim Menschen findet vor der 60sten Lebenswoche statt.

HALVERSON

EFFECTS OF PRACTICE UPON THE METABOLIC COST OF MENTAL WORK*

From the Psychological Laboratories of Columbia University

G. H. Rounds, H. J. P. Schubert, and A. T. Poffenberger

Experimental studies of the metabolic cost of so-called mental work have generally led to either one of two conclusions, namely, that the cost is significant in amount and comparable to the cost of certain kinds of physical work; or that the cost is so slight in amount as to defy measurement, or at any rate too slight to be significant. The latter finding is usually obtained where an attempt is made to isolate "pure mental work" by subtracting the cost to be attributed to the motor components of the work. For instance, the motor cost of answering examination questions has been determined in terms of copying words for an equivalent period, the motor cost of solving arithmetic problems in terms of writing figures equivalent to the answers to the problems, and the motor cost of learning nonsense syllables in terms of reading lists of nonsense syllables without intention or effort to learn them (1). It is assumed that the remainder is the metabolic cost of brain activity or of the mental work.

The earlier studies of muscular tensions during mental work and the more recent reports of the effects of artificially induced tensions upon mental work (6), together with the current concept of the functioning of the organism as a whole, suggest the futility of any separation of the mental from the muscular, or of pure brain activity from the activity of the whole organism.[1]

It is, indeed, probable that what are ostensibly the same reactions in putting down the answers to problems and simply writing equivalent numbers may be entirely different in degree of tension of the moving parts and in the accompanying reaction of other mechanisms. Becker and Olsen (1) found, for example, that writing nonsense syllables with no attempt to learn them increased the total metabolism 2 to 3%. When the same writing movements were

---

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, January 13, 1932.

[1] The technique recently applied by Myerson (12) gives promise of adequate data on the metabolism of the brain during mental activity.

made and the syllables were learned, the total metabolism was raised as much as 25%.

The results of previous work on metabolic cost have been conflicting in some cases because of the failure to specify or control the difficulty of the task and to measure the speed or rate at which work is performed. These two factors, equivalent to load and rate in mechanical operations, are fundamental in determining the cost of work. It has been shown by Becker and Olsen (1) that learning a list of 8 nonsense syllables cost a certain subject 12% excess of CO_2 per unit of time over the resting value, while a 12-syllable list cost 15% excess, and a 20-syllable list cost 25% excess. The subjective estimate of difficulty in this case was paralleled by increased cost per unit of time. The rapid increase in metabolic cost with increase in rate of work is well known in the case of physical work, as in different rates of walking and running (9).

A psychologically acceptable statement of the problem of the metabolic cost of mental work would seem to be: What is the metabolic cost to the organism of doing a given kind of work, such as adding or multiplying figures under specified conditions? The statements might, for convenience, be shortened by using the term "mental work" when the muscular component is, superficially at least, slight. Such abbreviated statements are, however, dangerous, as they may lead to the assumption that brain activity alone is being measured.

To avoid misunderstanding in studies of this kind it should be stated that doing "mental work" generally or always means doing *a certain job* or producing a certain output of *useful work* definitely prescribed, to distinguish it from useless work, such as worrying whether one is making too many errors, holding attention to the task against distracting circumstances, etc. A unit of worry may conceivably cost more than a unit of useful output in problems solved, but from the point of view of the organism at work the former will be charged as cost against the latter. That is, if doing a given job under given circumstances engenders worry, or necessitates resisting distracting influences, then each unit of output will bear an increased cost. The procedure here is similar to that of charging against the finished product the cost of friction in a lathe or other mechanical device.

The present experiment will be concerned mainly with the changes in the cost to the organism of adding columns of figures (which in the usual terminology is called "mental work") during the course of practice, and will accept as a starting point the view that mental work uses energy. The units of output will be objectively standardized, objective working conditions will be kept constant, work done will be measured in terms of rate or number of units completed, and cost will be measured in terms of energy expended.

By way of historical background the investigation of Becker and Olsen (1) should be mentioned. They measured the effects of practice upon the cost of learning nonsense syllables. Measurements were in terms of cubic centimeters of CO_2 produced per second of working time. They report that practice reduces the number of readings required to learn a test, increases the number of lists learned in a given time, and reduces the metabolic cost per unit of time. Thus, one subject learned 3 16-syllable lists in 1440 seconds, reading each list 18 times and with a CO_2 cost over resting of 1.48 c.c. per second. One month later after considerable practice he learned 6 16-syllable tests in 1520 seconds, reading each list 12 times and at a CO_2 cost of 1.38 c.c. per second. Therefore, the amount of work done in approximately the same time was increased 100%, while the rate of CO_2 production decreased slightly (about 7%). These authors attributed this increased cost over resting to brain activity, since they found that merely reading nonsense syllable lists costs very little (in one subject 3.6 c.c. of CO_2 per minute).

Technique of the Experiment

The unit of mental work consisted in the addition of 10 3-place numbers. The problems, which were arranged 20 to a sheet,[2] were made as nearly equivalent as possible in initial difficulty and in the amount of practice upon the various number combinations. Performance was measured in terms of number of problems solved and no account was taken of errors. To insist upon an errorless record would have introduced a complicating factor into the situation whose effect could not be predicted, while to penalize for errors according to some arbitrary rule seemed inadvisable at this stage of our work. The whole question of the cost of accuracy of performance in such work as this constitutes a problem in itself.

---

[2] In later work each problem was printed on a separate card and the answers recorded in columns on a sheet. The cards were shuffled at the beginning of each test period. This procedure is preferable for several reasons.

Metabolism was measured by means of the Douglas bag method, using a Bailey mask, Bailey valves, the Haldane-Henderson-Bailey analyzing apparatus, and the Bailey sampling bottles.[3] Volume of expired air was measured by means of a dry gas meter.[4] Samples of expired air were analyzed for oxygen and carbon dioxide content and, in addition to these values, the respiratory quotient, total ventilation, and calories per square meter of body surface were computed. A graphic record of respiration, showing rate and volume, was also obtained by way of a large tambour communicating with the column of expired air.

The subject came to the laboratory at 11 A.M., about three hours after eating. He rested on a cot for one-half hour, then seated himself in the apparatus and had the mask attached. After several minutes' rest in this position, a 10-minute sample of expired air was taken to furnish the "resting" metabolic rate. At a given signal the subject began adding and continued at his best rate for 20 minutes. Expired air was collected separately for the first and second 10 minutes. Following this 20-minute period and during the period of "recovery" the expired air was collected in two samples varying in length from 1½ to 9 minutes. Thus, for each experimental sitting the following specimens of expired air were available; resting, first and second half of work period, first and second half of recovery period. These 5 specimens entail approximately five hours of analysis[5] aside from the time required to make the various computations.

The data are given in Table 1 for each separate day and for the rest period, the two work periods, and the recovery periods. Under each of these heads there is given the total ventilation, or volume of expired air in liters per minute, the respiratory quotient (*R.Q.*), the oxygen consumed in cubic centimeters per minute, and the calories per hour per square meter of body surface (*Cal.*). Under the head of *Total net* is the total cost of the 20 minutes' work in excess of the resting cost.[6] Under the head of *Net per unit* is the cost over the

---

[3] A description of these devices will be found in (8).

[4] In subsequent experiments a wet laboratory gas meter was employed for greater accuracy, although no gross errors were detected in the meter used here.

[5] Two analyses are made on separate analyzers for each specimen to provide a check against errors in technique.

[6] Computed on basis of average resting rate. See discussion of resting metabolism.

TABLE 1
Changes in Metabolic Cost with Practice

		Rest	Work I	Work II	Recovery I	Recovery II	Total net	Net per unit	No. problems
Mar. 13	Vent.	8.00	9.72	9.83	....	....			
	R.Q.	.76	.82	.82	.69	.69			22
	O_2	230.00	282.90	295.00	200.00	233.90	1525	69	
	Cal.	38.60	48.14	50.25	33.10	38.70			
Mar. 15	Vent.	7.28	8.96	8.96	9.36	5.81			24
	R.Q.	.77	.80	.81	.77	.68			
	O_2	215.80	289.30	294.80	302.50	201.70	1587	66	
	Cal.	36.30	49.16	49.93	50.86	33.36			
Mar. 18	Vent.	6.84	8.96	9.16	7.90	6.54			24
	R.Q.	.78	.80	.81	.73	.69			
	O_2	224.60	281.00	289.70	252.70	236.00	1453	60	
	Cal.	37.87	47.60	49.20	42.06	39.20			
Mar. 20	Vent.	7.36	8.92	9.53	7.64	7.23			26
	R.Q.	.78	.84	.84	.76	.74			
	O_2	230.00	279.20	297.60	268.00	232.70	1514	58	
	Cal.	38.76	47.80	50.95	44.95	38.83			
Mar. 22	Vent.	7.42	8.32	8.97	7.09	8.38			25
	R.Q.	.79	.81	.79	.71	.72			
	O_2	227.00	264.50	290.00	252.00	270.70	1299	52	
	Cal.	38.33	44.90	49.00	41.68	44.93			
Mar. 25	Vent.	8.08	9.57	8.83	6.16	8.40			29
	R.Q.	.77	.81	.77	.67	.72			
	O_2	220.00	282.40	287.00	248.00	262.30	1440	50	
	Cal.	36.97	48.00	48.24	41.07	43.50			
Mar. 27	Vent.	6.81	8.76	8.62	5.50	6.08			27
	R.Q.	.77	.80	.77	.69	.74			
	O_2	199.60	275.20	276.00	232.70	190.40	1258	56	
	Cal.	33.57	46.62	46.40	38.50	31.76			

TABLE 1 (*continued*)

		Rest	Work I	Work II	Recovery I	Recovery II	Total net	Net per unit	No. problems
Apr. 3	Vent.	6.54	8.97	7.46	6.02	6.83			30
	R.Q.	.77	.83	.77	.67	.72	675	23	
	O_2	204.10	259.30	233.60	245.10	222.10			
	Cal.	34.32	44.28	39.27	40.55	36.86			
Apr. 5	Vent.	6.53	8.63	8.41	6.20	6.26			31
	R.Q.	.77	.80	.79	.71	.69	1044	34	
	O_2	199.10	261.30	268.30	230.30	220.40			
	Cal.	33.56	44.30	45.33	38.14	36.46			
Apr. 8	Vent.	6.64	8.86	8.32	5.93	7.14			31
	R.Q.	.75	.77	.72	.64	.69	1193	39	
	O_2	212.10	267.60	277.10	241.60	233.50			
	Cal.	35.47	45.00	46.12	39.96	38.63			
Apr. 10	Vent.	7.15	9.10	8.77	7.08	7.26			28
	R.Q.	.76	.77	.75	.73	.65	1507	54	
	O_2	207.90	289.40	286.70	231.60	246.20			
	Cal.	34.10	48.67	47.97	38.53	40.70			
Apr. 12	Vent.	7.57	9.70	9.48	5.51	6.92			32
	R.Q.	.75	.81	.77	.68	.71	1645	52	
	O_2	227.90	295.00	294.90	221.30	248.50			
	Cal.	38.07	50.12	49.60	36.60	41.15			
Apr. 15	Vent.	6.87	8.94	9.91	9.42	7.77			29
	R.Q.	.68	.80	.81	.75	.73	1709	58	
	O_2	219.80	283.40	312.80	236.30	243.10			
	Cal.	36.36	48.02	53.15	39.60	40.46			
Apr. 17	Vent.	6.12	7.68	8.69	6.95	....			33
	R.Q.	.77	.83	.82	.78	....	1026	31	
	O_2	193.30	245.00	283.00	223.00	....			
	Cal.	29.24	41.83	48.13	37.60	....			
Apr. 19	Vent.	8.23	9.03	9.71	7.50	....			31
	R.Q.	.77	.82	.80	.76	....	1793	58	
	O_2	225.16	287.30	317.40	234.60	....			
	Cal.	37.95	48.93	54.06	40.27	....			

TABLE I (*continued*)

		Rest	Work I	Work II	Recovery I	Recovery II	Total net	Net per unit	No. problems
Apr. 22	Vent.	8.46	8.58	9.12	6.86	....			34
	R.Q.	.80	.84	.82	.74	....			
	O_2	248.20	252.40	322.90	238.00	....	1499	44	
	Cal.	39.97	43.20	55.00	39.70	....			
Apr. 24	Vent.	7.56	8.68	8.39	9.15	5.36			34
	R.Q.	.74	.78	.79	.72	.68			
	O_2	236.60	273.30	287.60	311.00	217.00	1355	40	
	Cal.	39.50	46.10	48.60	51.52	35.90			
Apr. 26	Vent.	7.02	8.51	8.93	6.38	....			34
	R.Q.	.75	.83	.81	.78	....			
	O_2	226.20	283.30	301.50	238.80	....	1594	47	
	Cal.	37.60	47.77	51.16	39.50	....			
Apr. 29	Vent.	6.68	8.06	8.61	6.52	6.77			32
	R.Q.	.67	.76	.78	.70	.73			
	O_2	215.10	254.80	292.80	237.90	207.10	1222	38	
	Cal.	35.60	42.94	49.10	39.26	34.46			
May 1	Vent.	6.39	9.19	9.70	6.03	6.50			35
	R.Q.	.68	.84	.84	.66	.70			
	O_2	205.80	267.40	288.20	229.60	208.80	1302	37	
	Cal.	34.08	45.54	48.20	39.00	34.54			
May 3	Vent.	6.87	7.75	9.48	6.27	6.85			38
	R.Q.	.73	.80	.84	.68	.72			
	O_2	209.00	265.00	308.80	237.00	224.10	1484	39	
	Cal.	34.78	44.90	52.90	39.34	37.20			
May 6	Vent.	6.29	9.67	9.47	6.26	6.43			35
	R.Q.	.73	.84	.86	.66	.72			
	O_2	198.40	283.30	272.10	241.00	205.00	1310	38	
	Cal.	33.02	48.50	45.94	39.76	34.03			
May 8	Vent.	7.40	9.52	9.12	6.68	7.65			39
	R.Q.	.74	.85	.80	.67	.72			
	O_2	227.20	289.00	289.20	247.20	237.10	1528	39	
	Cal.	37.90	49.60	49.00	40.93	39.35			

TABLE 1 (*continued*)

		Rest	Work I	Work II	Recovery I	Recovery II	Total net	Net per unit	No. problems
May 10	Vent.	6.77	10.00	9.47	4.82	7.61			39
	R.Q.	.71	.87	.83	.69	.72			
	O_2	201.10	308.40	288.80	176.30	233.00	1718	44	
	Cal.	33.30	53.20	49.32	29.14	38.66			
May 13	Vent.	6.54	8.28	8.49	7.20	7.48			34
	R.Q.	.70	.83	.78	.72	.76			
	O_2	192.60	258.00	265.00	241.30	206.80	976	29	
	Cal.	31.86	44.04	44.66	40.04	35.07			
May 15	Vent.	6.53	9.53	9.56	5.63	6.42			40
	R.Q.	.72	.84	.85	.64	.65			
	O_2	205.00	279.60	273.10	220.30	217.60	1283	32	
	Cal.	34.03	47.90	46.56	35.58	36.30			
May 17	Vent.	6.64	10.33	9.00	5.08	7.49			41
	R.Q.	.77	.88	.83	.65	.72			
	O_2	197.90	296.50	268.10	203.70	233.60	1392	34	
	Cal.	33.30	51.27	45.80	33.30	38.77			
May 23	Vent.	7.20	8.37	7.87	7.10	....			34
	R.Q.	.82	.84	.83	.81	....			
	O_2	196.50	247.70	229.10	201.00	....	623	19	
	Cal.	33.71	42.40	39.10	34.13	....			
May 24	Vent.	7.17	9.15	9.34	6.75	....			28
	R.Q.	.83	.89	.86	.68	....			
	O_2	193.70	281.70	283.00	213.40	....	1393	50	
	Cal.	33.08	48.84	48.70	35.75	....			
May 27	Vent.	6.55	7.89	8.73	4.74	5.93			36
	R.Q.	.74	.83	.84	.64	.70			
	O_2	193.20	236.80	269.80	183.00	191.20	812	23	
	Cal.	32.22	40.44	46.20	30.17	31.62			

resting rate per unit of work, that is, per problem. These figures are given for oxygen only. Finally, the total number of problems solved during the 20 minutes of work is shown in the last column.

Resting Metabolism

One of the most difficult problems in studies of this sort, where one wants to determine the cost of work or of any set of conditions, is the need to establish a base or resting rate. Theoretically, at least, this rate should be uniform from day to day, if the subject's program of rest, diet, and work is under control. In practice, where one is not measuring basal metabolism, there are fluctuations due to a great variety of factors, some of which can and others cannot be controlled. The mere matter of adjusting the mask on the face, and the anticipation of the task to be performed, to mention no others, affect the metabolic rate. The subject who served in this experiment went through a preliminary adjustment period of 30 days, on each of which he was required to go through the test procedure except that he did not solve problems. Metabolism measurements made daily showed a very high rate at first with a gradual fall to the beginning of the experiment. Some indication that an adaptation might be occurring even during the 30 days of the test appears in a comparison of the average of the first five O_2 resting values (225.5 c.c. per minute) with the average of the last five (197.3 c.c. per minute).

The stability of our resting O_2 values (average 212.7 c.c. per minute A.D. 13.0) is not so great as that of data presented by Benedict and Cathcart (3) for a trained bicycle rider whose resting rate was taken over a period of 51 days (average 237.4 c.c. per minute A.D. 8.6).

Our variability is somewhat high because of the gradual fall of the resting rate from the beginning to the end of the experiment as noted above. Temporary fluctuations also play a part. For instance, on April 22, someone unexpectedly entered the laboratory while the experiment was in progress, whereupon the resting rate for that day rose to 248 c.c. of oxygen per minute. The rate was also high during the following experimental period (236.6), probably through apprehension of another interruption. Similar variations occur even in repeated records of basal metabolism. After an examination of the daily fluctuations in resting rate it was decided to base our calculations of *Net cost* upon the average resting rate of 212.7. Precedent for this procedure is found in the work of Krogh and Lindhard (10). From the psychological point of view, the variations in resting metabolism where objective conditions are kept fairly constant are of the greatest interest. Some of the factors creating irregularities in resting rate are being investigated at present.

Our resting rate is about 11% lower than the standard established by DuBois (5) for an individual of the same age, weight, and height as our subject. However, they conform closely to the figures given by Griffith (7) for two trained subjects whose rates were established by about 250 determinations. Their average rates fell 16.2% and 14.7% below the standards of DuBois.

COST OF MENTAL WORK

If the difference between the resting rate of O_2 consumption per minute and the rate for the first half of the work period be determined for each day, and these differences averaged, it is found that the work causes an increase of 61.2 c.c. of oxygen per minute (*P.E.* 2.4), or 28.8%. (The results are the same when computed on the average resting rate of 212.7.) The average difference between the daily records for the first and second work periods calculated in the same manner is 9 c.c. per minute, or 3% of the average for the first work period. This is not a reliable difference. In 11 of the 30 days the second work period has a lower rate than the first work period, whereas the first half of the work period never has a rate so low as that of the equivalent resting period. There is, therefore, no cumulative effect of any consequence passed on from the first half to the second half of the work period.

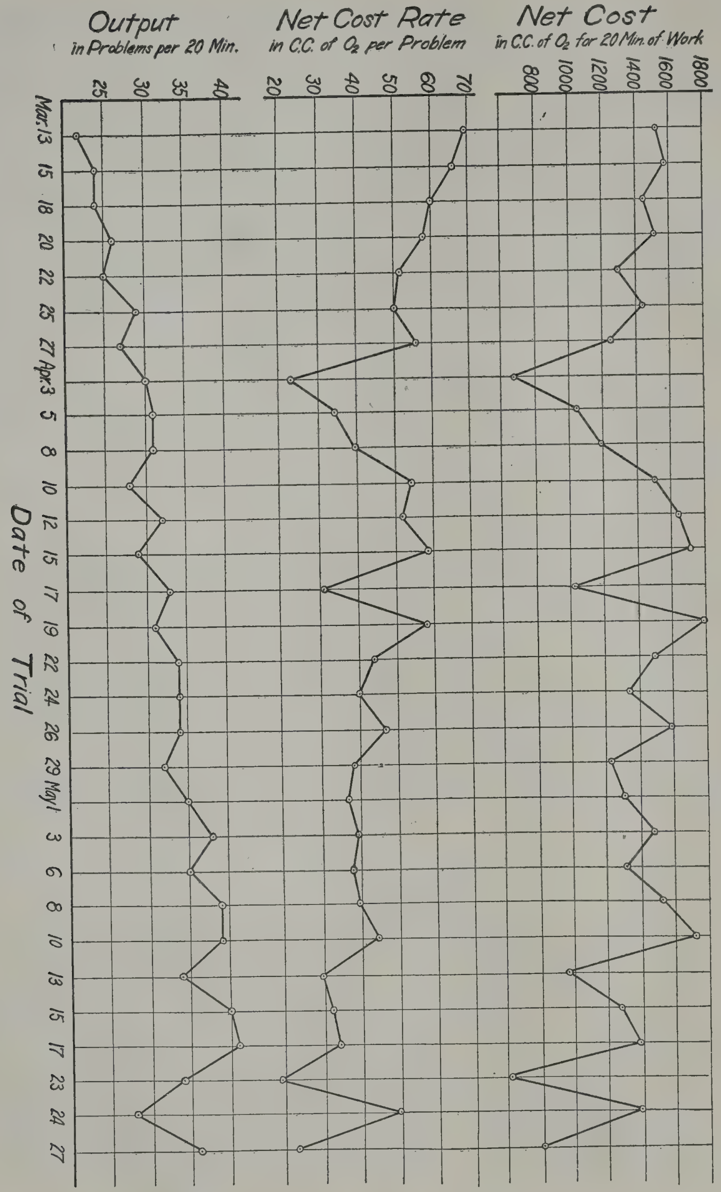
Strictly speaking, and arguing from analogy with the findings of the cost of physical work, the excess of oxygen consumed during recovery over the resting value should be charged to the cost of the work. In physical work of a strenuous character, the organism goes into debt, so to speak, for an amount of oxygen in excess of that which can be supplied from moment to moment. This deficit is made up during a longer or shorter period after the work has ceased. In order to determine the importance of this debt factor in mental work, the expired air was measured and analyzed following the second half of the work. Two samples were analyzed and the results are shown in the table under the head of recovery. Although these data are extremely interesting from the psychological point of view, they are difficult to treat in a statistical fashion. In the first place, the resting rate is seldom attained during the time over which the analyses are made, and, in the second place, the second recovery sampling may show a higher rate than the first one. Both these facts suggest that recovery is complicated by other factors which may or may not be legitimately interpreted as effects of the work period. Such attitudes as loss of interest after the work is finished, irritation at having to remain in the mask, as well as others gleaned from introspective reports undoubtedly affect the metabolic rate, but are only remotely, if at all, chargeable to the work. In the present study, therefore, it was decided to exclude the recovery records from the calculations of cost of work. A casual survey of the data in the table will show that the conclusions to be drawn from the investigation would not be modified by including the recovery data. The phenomena of recovery from mental work offer excellent opportunity for further research.

Effects of Practice

The table contains three measures which are significant for a consideration of the effects of practice, namely, the number of problems done in each 20 minutes of work; the net cost in oxygen for each 20 minutes of work; and the net cost in oxygen per problem for each day. These data are plotted in the accompanying figure (Figure 1). Each curve has its own vertical scale while the common horizontal scale represents the various experimental days.

If the average number of problems solved in the first five days (24.2) be compared with the average number solved during the last five days (35.8), there is a gain during the course of practice of 11.6 problems. Expressed in terms of the percentage of the first average, there is a gain of 46.7%.

The average net oxygen consumption per problem for the first five days is 61.0 c.c., and for the last five days is 31.6 c.c. This is a decrease of 29.4 c.c. per problem, or a gain of 48.2%. This figure may be taken as a measure of the real increase in efficiency, as it represents the amount by which the cost per unit of output was lowered as the result of practice. It appears, therefore, that practice not only increases the number of problems that can be solved in a given amount of time, but it increases also the ease with which the organism can solve them.


Output
in Problems per 20 Min.
25
30
35
40
Net Cost Rate
in C.C. of O_2 per Problem
20
30
40
50
60
70
Net Cost
in C.C. of O_2 for 20 Min. of Work
800
1000
1200
1400
1600
1800
Mar.13
15
18
20
22
25
27
Apr.3
5
8
10
12
15
17
19
22
24
26
29
May1
3
6
8
10
13
15
17
23
24
27
Date of Trial

An analysis of the task of adding the 10 3-place numbers in the problems shows that speed depends upon two factors, the number of pauses which the subject makes in passing from top to bottom of the columns or vice versa, and the length of each pause. It has been found (4) that a slow adder makes as many as 26 pauses in adding a column of 12 figures, and the total durations of these fixations is 68 seconds, whereas a rapid adder does the same task with 10 fixations which require only 7 seconds. The changes produced by practice are of the same order, the bulk of the gain being due to shortening the pauses, although some improvement undoubtedly results from decreasing the number of the pauses. Both of these changes result from learning the various number combinations, of which there are some 400 in our problems. The more thoroughly the sum of two numbers is learned, the more quickly will the correct answer come to mind, and the more readily can more than two numbers be added in one fixation of the eyes. To put the matter in simple terms, the task becomes easier or requires less effort.

The net cost in cubic centimeters of oxygen for each 20 minutes of work does not show a consistent decrease from beginning to the end of practice. The second half of the whole series shows a decrease of only 5.7% from the first half. While the last 5 days have an average about 25% lower than the first 5 days, the second set of 5 days is just about as low as the last set of 5, and the fifth set of 5 is about as high as the first set of five. In fact, the curve as plotted in the figure tends to remain level, although there are large fluctuations from point to point.

Caution should be exercised in drawing conclusions from the practice curves of a single individual, but one or two observations may be in order. If the total expenditure of energy tends to remain constant during a period of mental work while the output increases and the cost per unit decreases, we may have here the metabolic equivalent of the introspectively uniform state of "doing one's best." That is to say, doing one's best may mean not necessarily always doing the same amount of work, but expending the same amount of energy. This is suggestive of the steady state attained in the course of severe physical work. This suggestion is contrary to the conclusion of Morgan (11) that a subject in lifting weights tends to adjust energy expenditure to maintain uniform rate of work. This adjustment takes place within a single lifting reaction, or at most in the course of two or three lifts. On the other hand, our suggestion conforms with the finding of Vernon (13) that workers tend to adjust their efforts over a long period of time so as to give a certain total of output whether the working day be eight hours or ten hours. Here the rate of work is adjustable to some other condition which tends to remain constant, namely, the total amount of energy which the worker is willing to expend.

Out of this hypothesis of constant energy expenditure would arise the question as to how such constancy could be maintained. What would be the indicators of such energy expenditure? Some indirect indicators would have to be sought. Freeman (6) reports a high correlation between tonus and degree of effort. Perhaps consciousness of tonus, or the experience of effort, may be the whole or partial indicator. The irregularity of the "total net" curve from day to day, while maintaining a level throughout, would testify to the inaccuracy of the indicator. These and many other questions are raised by the simultaneous recording of output and cost, and can be resolved only by further research.

References

1. Becker, F. C., & Olsen, H. Metabolism during mental work. *Skand. Arch. f. Physiol.,* 1914, **31**, 81-197.
2. Benedict, F. G., & Carpenter, F. M. The influence of muscular and mental work on metabolism. *U. S. Dept. Agric. Bull.,* 1909, No. 208.
3. Benedict, F. G., & Cathcart, E. P. Muscular work. *Carnegie Instit. Publ.,* 1913, No. 187.
4. Buswell, G. T. Diagnostic studies in arithmetic. Chicago: Univ. Chicago Press, 1926.
5. DuBois, E. F. Basal metabolism in health and disease. Philadelphia: Lea & Febiger, 1927.
6. Freeman, G. L., & Lindley, S. B. Two neuromuscular indices of mental fatigue. *J. Exper. Psychol.,* 1931, **14**, 567.
7. Griffith, F. R., Pucher, G. W., Brownell, K., Klein, K., & Carmer, M. The metabolism and body temperature under basal conditions. *Amer. J. Physiol.,* 1928, **87**, 602.
8. Hawk, P. B., & Olaf, B. Practical physiological chemistry. Chap. 11 in *Energy metabolism,* by Dr. C. Bailey. Philadelphia: Blakiston's, 1927.
9. Hill, A. V., Long, C. N. H., & Lupton, H. Muscular exercise, lactic acid and oxygen. *Proc. Roy. Soc.,* 1924-1925, **97B**, 155—.
10. Krogh, A., & Lindhard, J. The relative values of fat and carbohydrate as sources of muscular energy. *Biochem. J.,* 1920, **14**, 290.
11. Morgan, J. J. B. The overcoming of distraction and other resistances. *Arch. Psychol.,* 1916, No. 35. Pp. 84.

12. MYERSON, A., & HALLORAN, R. D. Studies of the biochemistry of the brain blood by internal jugular puncture. *Amer. J. Psychiat.*, 1930, **10**, 389.
13. VERNON, H. M. The speed of adaptation of output to altered hours of work. *Indus. Fatigue Res. Board Rep.*, 1920, No. 6.

Columbia University
New York City

LES EFFETS DE L'EXERCICE SUR LE COÛT MÉTABOLIQUE DU TRAVAIL MENTAL

(Résumé)

On a trouvé que la tâche d'additionner les numéros de trois chiffres produisent un coût métabolique d'environ 30 pour cent de plus que celui du repos. On a établi de certains problèmes à l'égard du coût métabolique pendant le repos, et la moyenne des coûts des repos quotidiens pendant une série de pratique de trente jours a été choisie comme le coût le plus type pendant le repos. Cette série d'exercice, qui a suivi une longue période d'adaptation aux conditions de l'expérience, a donné parmi d'autres les mesures suivantes: le changement du nombre de problèmes résolus pour chaque unité de temps; le changement total de la quantité nette d'oxygène employée pour chaque période de travail de 20 minutes; et le changement de la quantité nette d'oxygène employée pour chaque problème. On a trouvé que le nombre de problèmes pour chaque unité de temps est devenu plus grand d'environ 45,0 pour cent, tandis que la quantité d'oxygène pour chaque problème est devenue plus petite d'environ 50 pour cent. La quantité nette totale empolyée pendant la période de travail de 20 minutes est restée relativement la même au cours de trente jours d'exercice. Bien que l'on doive se garder d'adopter comme vraie l'interprétation des courbes d'un seul sujet, on suggère une explication provisoire des données.

ROUNDS, SCHUBERT, ET POFFENBERGER

DIE EINWIRKUNG DER EINÜBUNG AUF DEN METABOLISCHEN AUFWAND BEI GEISTIGER ARBEIT

(Referat)

Es zeigte sich, dass bei der Aufgabe der Addierung dreistelliger Zahlen der metabolische Aufwand ungefähr 30% grösser war als bei der Ruhe. Es wurden gewisse Fragen angeregt in Bezug auf die Schnelligkeit des Metabolismus bei der Ruhe, und man wählte als die typischste Schnelligkeit bei der Ruhe den Durchschnitt der täglichen Schnelligkeiten bei der Ruhe im Lauf einer 30-tägigen Übungsserie. Diese Übungsserie, die einer langdauernden Periode der Anpassung an die Bedingungen des Versuchs folgte, leistete unter anderen folgende Messungen: die Änderung in der Zahl der per Zeiteinheit gelösten Aufgaben; die Gesammtänderung in dem Sauerstoffaufwand während der 20-minuten langen Arbeitsperiode; und Änderungen in dem Sauerstoffaufwand per Aufgabe. Man fand, dass die Zahl der per Zeiteinheit gelösten Aufgaben ungefähr 45% zunahm, während der Sauerstoffaufwand per Aufgabe ungefähr 50% abnahm. Der gesammte reine Sauerstoffaufwand während der 20-minutenlangen Arbeitsperiode blieb im Laufe der 30 Übungstagen relativ unverändert. Obwohl die en einer einzelnen Versuchsperson erhaltenen Kurven mit Vorsicht gedeutet werden müssen, wird auf eine vorläufige Erklärung der Befunde hingewiesen.

ROUNDS, SCHUBERT, UND POFFENBERGER

CONDITIONING AS A FUNCTION OF THE INTERVAL BETWEEN THE CONDITIONED AND THE ORIGINAL STIMULUS*[1]

From the Psychological Laboratories of The Ohio State University

HELEN MORRILL WOLFLE

HISTORY OF THE PROBLEM

The purpose of the following experiment was to determine the extent to which amount of conditioning is dependent upon the time interval between the conditioned and the original stimulus. A withdrawal movement of the finger, normally elicited by an electric shock, was conditioned to an auditory stimulus. Subjects conditioned with one time interval were not used on any other.

The essential features in the establishment of any conditioned response are that there be a stimulus effective in causing a response (the unconditioned stimulus), another stimulus to which the response is less definite or fixed (the conditioned stimulus), and that these two be presented in close temporal relationship to each other a variable number of times, but sufficiently often so that the conditioned stimulus elicits the response which originally followed the unconditioned stimulus. The conditioned response is then said to have been established.

Three temporal relationships of these two stimuli are logically possible: the conditioned stimulus may precede, may be simultaneous with, or may follow the unconditioned stimulus. Pavlov has given names to these relationships as follows:

1. *Trace conditioned reflexes,* in which the conditioned stimulus precedes the unconditioned, and the two do not overlap in time.
2. *Simultaneous or simple conditioned reflexes,* in which the two are given simultaneously, or at least in overlapping time. Simultaneous conditioned reflexes are supposedly the easiest to establish.
3. *Backward conditioned reflexes,* in which the conditioned

---

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, January 7, 1932.

[1] The experimental work for this manuscript was done under the direction of the late Albert P. Weiss, and the manuscript put into final form under the supervision of F. C. Dockeray.

stimulus follows the unconditioned. Since Pavlov first stated that with this temporal arrangement conditioning was impossible, in a sense he did not devise the name, which has come into use since his original work.

Pavlov, as late as 1927, as well as previously, has stated that conditioned responses will *never* result if the conditioned stimulus follows the unconditioned (21). However, in 1928, he reversed his position to some extent, as follows: " . . . if the (conditioned) stimulus is introduced after the beginning of the activity, then, although, as our present experiments seem to show, a conditioned reflex may also develop, it is insignificant and evanescent; on continuing the procedure the stimulus . . . becomes inhibitory" (22, p. 381). But this statement was published in a book having far less circulation than the earlier work. To demonstrate the result on the psychological literature, one has only to pick up any textbook with a chapter on the conditioned response. For example, in *The Foundations of Experimental Psychology* there is the statement: "If, for example, the presence of food in the mouth causes saliva to flow and if it is desired to produce that result with an auditory stimulus, the latter must either be presented with the food or slightly before the food. Presentation subsequent to the food will not result in the conditioned response" (10, p. 622).

Summaries of the other experiments dealing with the time interval between the conditioned and the unconditioned stimulus have been given in the papers of Switzer (26) and Wolfle (30). A brief review of the more pertinent ones follows.

Cason (2), with one adult subject, concluded that backward conditioning of the wink reflex was impossible.

Switzer (26), using 20 adult subjects, concluded that backward conditioning of the wink reflex was possible. In comparing his results with those of Cason, Switzer suggested that the lack of conditioning in Cason's experiment might possibly have been due to: fatigue of the subject; the fact that the conditioned stimulus was given during the refractory phase of the wink reflex; or the fact that the subject could voluntarily control the conditioned stimulus. Since Switzer himself failed to obtain conditioning in two of his 20 subjects, it seems that chance factors alone might explain the discrepancy of results.

Yarborough (31), using white rats, found that the simultaneous relationship was most effective in the formation of a conditioned withdrawal movement, but that backward conditioning was possible. The amount of conditioning varied inversely with the temporal separation of the two stimuli.

Schlosberg (25), studying the patellar reflex in eight adult subjects, found that intervals between .20 and .44 seconds, in which the conditioned stimulus was presented first, were most effective for conditioning. He did not investigate backward conditioning.

Hilgard (8), investigating the conditioned wink reflex with intervals between 400 and 0 sigma, the conditioned stimulus being presented first, noted that the intervals of 50 and 0 sigma were less favorable for conditioning than the others used.

Wolfle, in a previous study (30), using a withdrawal response, found that for 90 adult subjects the .50-second interval, with the conditioned stimulus first, was the most effective interval. It was also demonstrated that backward conditioning could be established, but to a lesser extent. These results will later be compared in more detail with those of the present experiment.

The Evaluation of Various Responses for Conditioning Experiments

The results of any experiment on conditioning are unquestionably influenced by the choice of the unconditioned reflex or response. The following factors should be considered before making the choice:

1. The intensity of the unconditioned stimulus should be subject to accurate control.

2. The extent or intensity of the unconditioned response should be as constant as possible. In Pavlov's experiments, the intensity of the unconditioned stimulus, the amount of food or the number of cubic centimeters of acid, was carefully controlled, but the effect of this stimulation could neither be as carefully controlled nor as accurately measured; for sometimes it is conceivable that the dog would lose part of the food. Also, the amount of chewing would influence salivation.

The reason for these first two demands is obvious. The strength of the conditioned response is often measured, not by the number of subjects in which it occurs, nor by the number of times a response is made to the conditioned stimulus, but by the amount or intensity of the conditioned response as compared with the intensity of the unconditioned response. If the unconditioned response in itself is not constant, the variation in the conditioned response might be caused by that factor, rather than by any instability of the conditioned response.

3. The unconditioned response should be one which is relatively unfatiguable. If this condition be not fulfilled, many more practice periods are likely to be necessary. For example, a common response to noises is orientation toward the source of the sound. However, this reaction quickly disappears with repetition of the sound; consequently, practice with it as the unconditioned stimulus might be limited to once or twice daily.

4. The reflex should be relatively isolated from complex conditions of reinforcement and inhibition. Pavlov states, for example, that the salivary reflex is relatively more isolated than striped muscle reactions. The difficulty is that when any reflex is investigated carefully, its constancy and isolation begin to appear a fiction. To quote a recent statement (5), "If the patellar reflex, commonly regarded as one of our commonest responses and one of our most automatic, shows the effect of unknown factors which are highly variable and not as yet subject to experimental control, it is clear . . . that the myth of the knee-jerk as a simple spinal reflex is shattered." Lashley (14, 15) and Winsor (28, 29) have shown the great complexity of the responses of the salivary glands in humans. Probably isolation of a muscle group or gland is a factor impossible of attainment in a reacting organism.

5. The unconditioned response should preferably be one which cannot be elicited by stimuli extraneous to the experiment. If this requirement be not fulfilled, the experimenter is never sure whether the obtained response is due to the effect of the supposed conditioned stimulus, or to some extraneous stimuli. The most difficult of these incidental stimuli to eliminate are those proceeding from the organism itself. Regardless of what external factors may be controlled, the dryness of the eyeball will cause the wink reflex to be given. With the salivary reflex, mouth movements might cause an increased secretion of saliva at the crucial time.

6. It should be possible to record the reflex objectively. Some responses are easier to record than others, and various measurements may be made. The pupillary reflex is difficult to measure objectively (although a continuous photographic technique might be employed). But for the more difficult technique involved, action currents in the muscles might provide a better measure of the knee-jerk than is provided by the mere measurement of the excursion of the foot.

7. The reaction-time to the unconditioned stimulus should be relatively short. If it is long, the causes of any phenomena will be more difficult to ascertain. In an experiment such as the present one, where the change in time intervals between the conditioned and the unconditioned stimulus was the principal variable, it was of particular importance to have an unconditioned reflex of short reaction-time.

8. The reflex should not be voluntary, that is, it should not be a response which has been previously conditioned to social stimuli. "Voluntariness" seems to be reducible to the fact that one can instruct oneself, or be instructed by another, to move a certain muscle or to secrete a glandular product. Thus some actresses have voluntary control of their lachrymal glands; boys bent on annoying teachers have their ear muscles under voluntary control; but for most of us, these actions cannot be performed at our own or at another's command. Now if a voluntary response is used for a conditioning experiment, such as the wink reflex or withdrawal from electric shock, the subject can decide how he is going to respond. If he thinks the experimenter will be pleased if he winks, and if he wants to please the experimenter, he can do so. The experimenter is also at liberty to discover a significant difference (if possible) between voluntary movements and conditioned movements in relation to reaction-time.

Whenever an attempt is made to condition a voluntary movement, the attitude imposed on the subject, either by the instructions given him or by his own self-instructions, is very likely to determine the outcome of the experiment. Unless the subject's attitude be known it is difficult to separate the effects of true conditioning from those of a cooperative attitude on the part of the subject.

When a voluntary movement is used some independent measure of the nature of the conditioned response should be employed. For example, Peak concluded: "The form of integration of the response may under certain experimental conditions differentiate reflex and voluntary responses" (23, p. 64).

No one of the various responses used in conditioning experiments satisfies all of these criteria.

The wink reflex is a fairly constant movement, which ordinarily does not fatigue to any appreciable extent; it is elicited by a relatively isolated muscle group; and it can be objectively recorded. The chief difficulty is that it may be a voluntary response, and it is also one that occurs frequently due to intra-organic stimulation, no matter what the experimental conditions are. The careful technique used by Hilgard in a recent experiment shows the possibilities of the reflex as a basis for conditioning experiments.

The patellar reflex has been carefully investigated (4, 5, 6), and the difficulty of eliciting a kick of constant extent is known. This difficulty, however, is probably no greater than that of securing a wink of constant extent or a certain amount of saliva. The knee-jerk can be simulated voluntarily. However, myograms or reaction-times might differentiate between a conditioned and a voluntary jerk.

The salivary reflex has received little favorable comment from those attempting to condition human subjects (Lashley, 14 and 15, and Winsor, 28 and 29). As previously mentioned, no matter how accurately the unconditioned stimulus is measured, the unconditioned response will necessarily vary with the manner in which this stimulus reaches the sense organs. In respect to the constancy of the salivary flow, Lashley has stated: "The gland is subject to almost constant stimulation from the receptors of the mouth and throat . . . Swallowing is seen to result in an accelerated flow, persisting for some minutes, and movements of the body also seem to have a slight stimulating effect . . . In some subjects, when particularly quiet, the activity of the parotid may cease completely for five or more minutes" (15, pp. 465-466).

The salivary reflex fatigues very little, particularly if the stimulation is strong. In three periods of 5, 10, and 20 minutes, Lashley reported that "the salivary reflex fatigues very slowly if at all" when the stimulation was a dilute solution of hydrochloric acid in two cases and a solution of sodium chloride in the other (15, p. 481).

With human beings as subjects, salivary secretions seem to be influenced by the general situation. Lashley has summarized his experiments in this paragraph: "Reflex secretion is excited by the sight and odor of food, but is conditioned by hunger, by the previous experience of getting food under the conditions of the experiment, and by complex emotional factors which have not been analyzed" (15, p. 492). Winsor writes: "The almost universal failure of investigators to condition the parotid secretions of human subjects is perhaps due to their failure to consider the dependence of this process upon favorable autonomic conditions. Unfavorable affective states expressed in moods, attitudes, or emotional states could, and perhaps have, blocked the development of the response" (29, p. 410).

The reflex is not voluntary in the ordinary sense; however, manipulations of the mouth and throat which are voluntary will result in increased salivation. It is entirely possible that any instructed subject could give an exhibition of conditioning of the salivary gland.

In summary, the statement can be made that as more facts are discovered relative to the salivary reflex, the more its simplicity seems to vanish.

The pupillary reflex can be elicited with known, standard stimuli; the rapidity and extent of the resulting movement may depend on factors such as fatigue and adaptation, but with proper experimental control should be quite constant from one test to the next. Cason reports that with proper precautions the eye does not fatigue. Watson, on the contrary, states: "The method is very uncomfortable for the subject. Indeed the long training necessary to produce the reflex in refractory cases would probably be actually injurious to the eyes" (27, p. 99).

Cason maintains that the change in size of pupil is easily observed with suitable apparatus. On the contrary, the lack of repetition of the experiment might point toward the difficulty of observation, or at least toward the difficulty of obtaining "suitable apparatus."

Cason obtained rather equivocal results in conditioning this reflex. Contraction was more definitely conditioned than dilatation. Watson, ordinarily optimistic in regard to conditioning, declared that "it is very difficult to obtain the (conditioned) reflex in it" (27, p. 99).

Withdrawal from electric shock (sometimes called simply the "motor reflex") has frequently been used as an unconditioned response. It, too, has both advantages and disadvantages. The intensity of the unconditioned stimulus, if controlled physically, has a variable effect. Partly as a consequence of this variability, the extent of the withdrawal movement is not constant.

Withdrawal from a painful stimulus is a response that is relatively unfatiguable. Its reaction-time is comparatively short. It is not ordinarily elicited by extraneous factors of the stimulus situation.

The withdrawal movement is not performed by an isolated muscle group, and does not even use the same muscle group from one time to the next. However, the greatest defect of the withdrawal movement as the unconditioned response is its voluntary character.

Other responses have been used with varying success in conditioning experiments, such as the shivering action when strong vinegar is placed in infants' mouths (18), the psychogalvanic response (11, 12), variation in leucocyte count in the blood (17, 19, 20), the reaching for food by infants (16), sucking responses (24), fear responses (3, 13), and so on. Other possible reactions that, as far as the author knows, have not been used include ear movement, the "dimpling" reflex of the abdominal muscles and stretch reflexes in general, and the movement of regurgitation.

In conclusion, no one response is without defects for use in a conditioning experiment. Extended work with all of them is doubtless the only method by which conclusions in regard to the conditioning of human beings can validly be made. In any particular conditioning experiment the unconditioned response should be selected with full recognition both of its advantages and disadvantages. The results of such experimentation should be accepted only when they have been corroborated by similar work using some other response which overcomes the particular defects of the first response used.

In the experiments reported in this paper, the withdrawal response was used. In spite of the objections to it which have already been enumerated, the withdrawal response seemed to be particularly adapted, due to its short reaction-time, to a study of the effect of time interval on conditioning. With such a method the results cited should be given acceptance only when other experimenters agree, and it has been pointed out that the work of other investigators (Switzer, Schlosberg, and Hilgard) are not in disagreement with this.

Apparatus and Method

The interval timer shown in Figure 1, which controlled the administration of both electrical and auditory stimuli, consisted of an endless belt of motion-picture film driven by a 1/100 H.P. General Electric synchronous motor. The speed of the motor was reduced from 1800 R.P.M. to 120 R.P.M. by a 30 to 1 worm reduction unit and spur gearing in the ratio of 1 to 2. Thus 120 R.P.M. was the speed of the driving sprocket (labelled *B* in the illustration). The length of the film, 29 feet, 1.75 inches, was 120 times the circumference of the sprocket, in order that the film might make one complete revolution per minute. For greater compactness

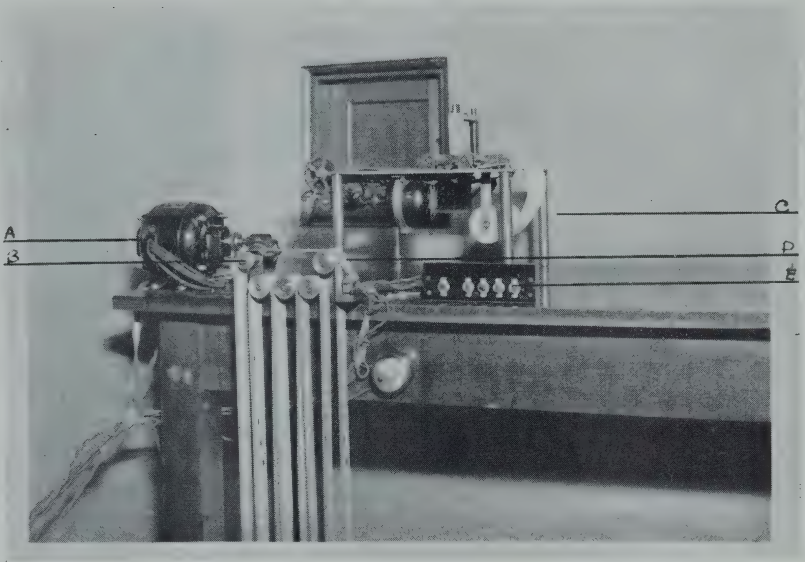


FIGURE 1
THE APPARATUS
A—motor; *B*—driving sprocket; *C*—polygraph; *D*—rolling contacts; *E*—switches.

this film was run over a series of idler pulleys (the upper ones appear in the picture) which allowed the film to double back on itself several times. Outside of one of these pulleys, and separated from it by the film, were five small rolling contacts (*D*). One of these was connected into an electric circuit which included: a 6-v. storage battery; the primary coil of an inductorium; one pen of an electrically recording polygraph; and the idler pulley against which the contact roller was running. Any perforation in the film which would allow the roller to make contact with the pulley beneath would then complete this circuit and administer an electric shock to the subject, and, at the same time, record on the polygraph. The other four rollers were connected in parallel into a similar circuit, which included an electric sound hammer instead of the inductorium. The auditory stimulus was given and that fact recorded on the polygraph whenever this circuit was completed by a similar perforation in the film.

Any time interval of less than one minute could be obtained by making the two perforations in the film at the proper distance from each other. As the driving motor was of the synchronous type, the film ran at a highly constant rate. It was thus possible to obtain constant time intervals simply by translating the desired time into linear distance on the film. As there were four rollers connected in parallel in the auditory stimulus circuit, four distinct time intervals could be obtained on the same film. Any one of these could be used simply by closing the appropriate switch. More than four intervals were obtained by using similar pieces of film with differently located perforations.

As it was thought that several pairs of stimuli could advantageously be presented in each minute, five such groupings of perforations were made in each length of film. These also could be given any temporal spacing desired by making the groups of perforations at the proper distance from each other. In terms of seconds, the intervals between pairs of stimuli were 12, 8, 15, 9, and 16 seconds for this experiment.

The sound for the conditioned stimulus was produced by the tapping of a brass-tipped rod on an iron plate, the rod being the armature of a solenoid.

The shock (the unconditioned stimulus) was the current induced in the secondary coil of an inductorium when the interval timer completed the 6-volt D.C. circuit to the primary coil. The shock was delivered to the subject through two brass electrodes. One of these was a 3-by-4-inch brass plate mounted on a wooden arm-rest. The palm of the hand rested on this electrode. The other was a 1-inch square of brass mounted on the end of a spring brass arm. The subject rested the ball of the middle finger on this electrode. When the subject raised his finger the spring brass arm completed the recording circuit to the polygraph.

Seventy-two students in elementary psychology comprised the experimental group; none of these had previous information as to the nature of the experiment. In addition, six subjects served in a control group. Each subject was given a demonstration of the correct method of placing his hand and finger on the electrodes, and was instructed to lift his finger when shocked, and to replace it after the shock was over. No subject was used on more than one interval.

Practice periods for each subject were 40 minutes in length; thus 200 pairs of stimuli were presented during each period. The subjects came from three to six times, and sometimes even more frequently.

The conditioned stimulus was never presented alone during the first 20 minutes of any practice period, and only 10 times at irregular intervals during the last 20 minutes. Thus, 30% of conditioning for one practice period would mean that the subject lifted his finger 3 of the 10 times on which he was tested.

For a few subjects who acted as a control group, the conditioned stimulus was not given alone until the fourth practice period.

The unconditioned stimulus (shock) was constant for each subject in the sense that its intensity was just sufficient to cause a withdrawal movement; however, the absolute intensity varied from subject to subject, and in general was gradually increased throughout each 40-minute period.

The subject sat in a room by himself, the only apparatus in it being the electric hammer and the device for administering the shock. The interval timer and recording apparatus were in an adjoining room.

RESULTS

The raw data are given in Table 1 and are summarized in Figure 2 (the curve labelled A). The abscissa values are the time intervals

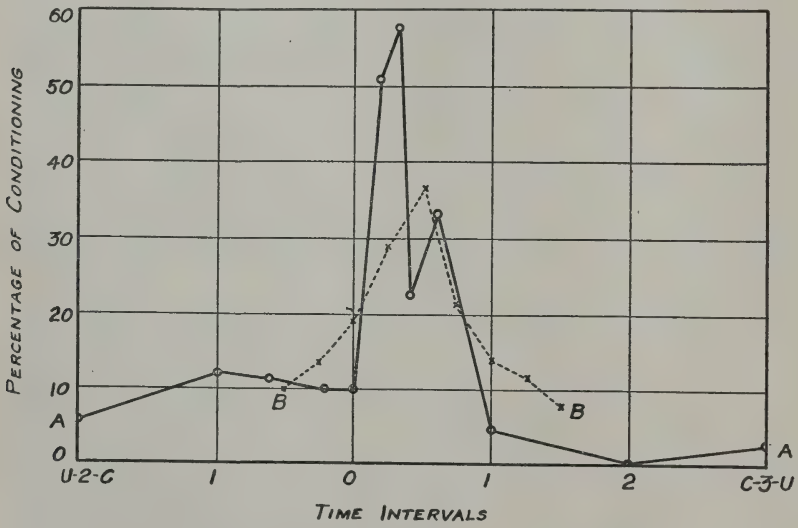


FIGURE 2
RESULTS OF TWO EXPERIMENTS
A—data of the present experiment; *B*—data of the previous experiment. U-2-C—unconditioned stimulus, 2-second interval, then conditioned stimulus.

TABLE 1
Percentage of Conditioning

Practice periods	1	2	3	4	5	6	7	8	9	Mean, 3 practices	Mean, all data
Interval U-2.0-C											
	0	0	0							0	0
	0	0	0							0	0
	0	0	0							0	0
	0	0	0	0	0	0				0	0
	30	40	0	10	20	0	30			23	19
	10	20								15	15
							Mean			6	6
U-1.0-C											
	40	0	0	0						13	10
	0	0	0	0						0	0
	0	10	0							3	3
	70	30	0	0	0					33	20
	30	10	0	30						13	17
							Mean			12	11
U-0.6-C											
	0	10	40	33	30	30	10	30		16	23
	50	20	0	0	0					23	14
	10	0	30	10	0	10	0			13	8
	0	0	0	0	0					0	0
	0	0	10	0						3	3
							Mean			11	10

TABLE 1 (*continued*)

Practice periods	1	2	3	4	5	6	7	8	9	Mean, 3 practices	Mean, all data
Interval U-0.2-C	0	0	0	0	0					0	0
	0	0	20	0						7	5
	0	10	0	0	0					3	2
	20	30	40	10	20					30	24
	30	0	0	0	0					10	6
							Mean			10	7
U-0.0-C	40	20	0	10						20	18
	0	10	0	0						3	3
	60	70	20	0	0					50	30
	0	0	0	0	0					0	0
	0	0	0	0						0	0
	0	0	0	0						0	0
	0	0	0	0						0	0
							Mean			10	7
C-0.2-U	10	10	0	0	0					6	4
	30	20								25	25
	100	70	80	100	70	60	100			83	81
	60	80	100	90	100	100				80	88
	60	10	80	90						50	60
	100	100	80	60						93	85
	40	20	10							23	23
							Mean			51	52

TABLE 1 (*continued*)

Practice periods	1	2	3	4	5	6	7	8	9	Mean, 3 practices	Mean, all data
Interval C-0.3-U	100	100	100	100	10	50	0	10	0	100	52
	80	60								70	70
	40	0	0	0	0					13	8
	0	0	0	0	0					0	0
	60	90	80	50	40					77	64
	100	100	100	100	100					100	100
	80	40	10	20						43	38
							Mean			58	47
C-0.4-U	90	70	90	70						83	80
	0	0	10	10						3	5
	0	40	60	90	70					33	52
	20	70	0	80						30	43
	10	0	0	0						3	3
	10	10	10	0						10	8
	0	0								0	0
							Mean			23	27
C-0.6-U	10	0	0	0						3	3
	20	30	0	0	0					17	10
	90	90	100	100	100	100				93	97
	0	10	50	80						20	35
	100	100	100	100						100	100
	10	0	0	0						3	3
	0	0	0	0						0	0
							Mean			34	35

TABLE 1 (*continued*)

Practice periods	1	2	3	4	5	6	7	8	9	Mean, 3 practices	Mean, all data
Interval C-1.00-U											
	0	10	0	0						3	3
	40	10	20	40						23	28
	10	0	0	0						3	3
	0	0	10	30						3	10
	0	0	0	0						0	0
	0	0	0	0						0	0
							Mean			5	7
C-2.00-U											
	0	0	0	0						0	0
	0	0	0							0	0
	0	0	0							0	0
	0	0	0							0	0
	0	0	0							0	0
							Mean			0	0
C-3.00-U											
	0	0	0							0	0
	0	0	0							0	0
	0	0	0							0	0
	0	0	0							0	0
	20	10	10	0						13	10
							Mean			3	2

TABLE 1 (*continued*)

CONTROL GROUP—EFFECT OF UNREINFORCED CONDITIONED STIMULUS

Practice periods	1	2	3	4	5	Mean
Interval C-0.3-U						
	no test	no test	no test	100	100	100
	no test	no test	no test	100	100	100
	no test	no test	no test	60	90	75
	no test	no test	no test	10	0	5
	no test	no test	no test	10	0	5
	no test	no test	no test	100	100	100
				Mean		64

used. The ordinate values are the average percentages of conditioning for each interval (combining the first three practice periods of all subjects).

The computations are based only on those subjects who came for at least two periods. As is evident from a glance at the table more subjects were used on those intervals in which the conditioned stimulus preceded the unconditioned by from 0 to 0.6 seconds. On these intervals the subjects were also asked to come more frequently.

From the left side of the curve it may be seen that on those intervals in which the unconditioned stimulus was presented first very few conditioned responses occurred. The actual percentages varied from 6 to 12.

Switzer (26) has reported an experiment on backward conditioning in which approximately the same tendencies were demonstrated. Switzer states that "conditioned lid reactions were secured in 18 out of 20 subjects (90%)" (p. 95). If the data of the present experiment be considered in the same way, it may be shown that 14 out of 21 (67%) were conditioned to some extent. But of course such a statement gives no indication of the stability of the conditioning.

TABLE 2

DATA FROM SWITZER'S EXPERIMENT, ARRANGED BY TIME INTERVALS

Time interval in seconds	Number of reinforcements necessary to produce conditioning	Mean for each interval
2.0	11	
	14	
	— (no c. r.)	
	— (no c. r.)	——
1.0	20	
	17	
	17	
	24	
	20	
	47	
	62	
	12	
	30	
	58	31
0.75	20	20
0.50	20	
	46	
	25	
	23	
	7	24

It is impossible to tell from the data given by Switzer the frequency of occurrence of the conditioned responses. However, Table 1 (26, p. 83) in his article can be rearranged to show the relative merits of the different time intervals used.

Conditioning in Switzer's experiment was measured by "the number of reenforcements to produce 3 or more successive conditioned responses to buzzer" (p. 83). Hence the more reinforcements necessary, the greater the difficulty of conditioning, presumably.

It is evident from the table that the 0.5-second interval required fewer double stimulations on the average than did the 1.0-second interval. However, the difference is not statistically significant. These are the only intervals that can be compared with each other, as only one subject was used on the 0.75-second interval; and the results from the 2.0-second interval do not give a mean.

The two who were never conditioned in Switzer's experiment came from the 2.0-second interval, the longest one used. In the present experiment, 57% of those in the backward intervals who were never conditioned came from the 2.0-second interval.

It might be suggested that Switzer's technique resulted in a greater percentage of conditioned responses than did the technique of this experiment. His method of testing for conditioning was as follows: "First evidences of the onset of conditioning were usually manifested by the appearance of double winks for the paired stimuli —a complete wink or closure for the striker followed by a partial closure for the buzzer. When this phenomenon appeared, the experimenter sounded the buzzer alone to determine whether the conditioned reflex had been established. Three or more consecutive responses to the buzzer alone were accepted as evidence that a conditioned eyelid-reaction to the sound had been established" (pp. 83-84). On the other hand, in the present experiment the subjects were tested at a definite time, rather than at a certain point of progress. Also, the testing was done in the latter half of the experimental period rather than the first half, because the subjects were then more adapted to the situation and less disturbed by incidental stimuli.

In summary, it can be said that most experimenters have found backward conditioning to be possible; but the percentage of conditioned responses occurring is likely to be small. Although the evidence is rather slight, it is probably also true that the amount of conditioning increases as the interval between the unconditioned and the conditioned stimulus grows smaller.

The zero interval, or the simultaneous relationship, produced very little conditioning (10%). This result is contrary to the usual impression regarding the effectiveness of the interval, but is supported by the work of Schlosberg (25) and Hilgard (8), as well as by the previous experiment of Wolfle (30). Since Pavlov also designated as simultaneous those intervals which were "almost simultaneous" his results cannot be exactly compared. Yarborough (31), however, found the simultaneous interval to be the most effective in conditioning a turning response of rats. However, such an experiment might well yield different results from the conditioning of the wink reflex or other responses in human beings.

Beginning with the 0.2-second interval in which the conditioning stimulus was first presented, there is a sharp rise in the curve. This higher level holds, with minor fluctuations, until the 0.6 interval. But with the 1.0-second interval the curve has again reached a level which represents a very much smaller amount of conditioning.

Considering the middle section in detail, it is apparent that the 0.3-interval results in slightly more conditioning than the 0.2-second interval. That this difference was insignificant is shown by the critical ratio. Most subjects came more than 3 times, but in order to make the several points on the curve comparable it seemed preferable to use only the number of practices common to all. However, if all the practice periods be included, the percentages change very slightly (from 0 to 4% in the case of any one of the intervals except the 0.3-second one, which drops to 47%, a difference of 11). Using the data from all the practice periods, then, the 0.2-second interval was the optimal one for conditioning; but using only the data from the first three practice periods, the 0.3-second interval was the optimal one.

Although there are minor fluctuations in this curve, it can be divided into three reliably different sections: (*a*) the intervals with the conditioned stimulus last, which never resulted in stable conditioning and frequently in none at all; (*b*) a band of intervals between 0.2 and 0.6 seconds, with the conditioned stimulus first, which gave stable conditioning with many subjects; and (*c*) those intervals longer than 0.6 seconds, with the conditioned stimulus first, which resulted in no stable conditioning with any subject, and, with many subjects, in no conditioning at all.

Curve B in Figure 2 gives the results of another experiment (Wolfle, 30) using the same withdrawal responses, the same unconditioned and conditioned stimuli but with slightly different time intervals and with different technique. The subjects came once only for periods of 55 minutes in length, and, during this time, 3 out of every 20 stimulations were with the conditioned stimulus alone. Ninety subjects, 10 for each interval, were used. The greater number of subjects, as well as chance factors, would account for the greater smoothness of the curve. The same general conclusions can be drawn. The region between 0.25 and 0.5 seconds, with the conditioned stimulus first, was most effective for conditioning, with the curve dropping rather rapidly on either side of this region.

Effect of Frequency

According to the usual report, a certain amount of repetition is necessary in order to form a conditioned response. A typical statement is: "Experiments on conditional reflexes make us think that frequency is one of the important factors underlying conditioning" (10, p. 622). However, under the conditions of this experiment, the percentage of conditioning became less on the average rather than greater, as practice continued. The subjects, it will be recalled, came three times a week for 40-minute periods. Of the 72, 25 were never conditioned. Of the remaining 47 subjects, 18 showed no constant change; 8 showed an increase in conditioning from one practice period to the next, and 20 showed a decrease in conditioning. These figures warrant at least the statement that conditioning does not improve with practice.

It is questionable whether this decrease in amount of conditioning as practice continued was a result of experimental extinction (due to the unreinforced test stimulus, which occurred ten times per practice period) or of some other factor such as a possible adaptation to the entire situation. In an attempt to settle this point, six subjects (using the 0.3-second interval with the conditioned stimulus first) were never tested until their fourth and fifth practice periods.

The average of these last two practice periods was higher than the average of the 0.3-second interval under the other technique, that of testing the subjects 10 times every day. The exact percentages are .64, with the testing on the last two days only, versus .58, with the testing on the first three days (an unreliable difference). If the comparison be made between the fourth and fifth day's performance of the control group with the fourth and fifth day's performance of the group using the regular technique, the difference is greater, being

64% for the control and 38% for the usual group. It seems safest to conclude that the decrease in amount of conditioning was of the nature of experimental extinction, even though only 5% of the stimulations were not reinforced by the shock.

Three of the six subjects in the control group were conditioned to the extent of 100%. No other group had that high a number of subjects perfectly conditioned, even when just the first practice period is considered. If the conditioned response technique is to be used for experimental purposes other than that of merely discovering whether conditioning is possible or not (the usual subject of articles), and if a stable and maximum amount of conditioning is desired (it being difficult to obtain stability without having either the maximum or the minimum), it would seem preferable not to give the unreinforced conditioned stimulus for some time after the experiment has begun.

The facts discovered relative to conditioning finger withdrawals from electric shock may not apply to other types of response. But, if general conclusions be permitted one, they would follow these lines:

At best, it is difficult to secure stable conditioning in adult human beings; but the optimal conditions are an interval of 0.2 to 0.4 seconds between the conditioned and the unconditioned stimulus, with no testing for conditioning by means of presenting the unreinforced conditioned stimulus until some time after the experiment has begun.

CONCLUSIONS

In conditioning a withdrawal movement of the finger to an auditory stimulus, the number of conditioned responses was influenced by the time intervals between the conditioned and the unconditioned stimulus.

1. Backward conditioning was found to be possible, but the percentages, from 6 to 12, were very low.
2. Forward conditioning was very small in amount except with the intervals between 0.2 and 0.6 seconds; with these the percentage of conditioning was as high as 58.
3. In this experiment, practice did not increase the number of conditioned responses occurring, and, on the average, even decreased this number.
4. When no test was made of conditioning for the first three practice periods, the amount of conditioning was increased in the

latter periods. This would indicate that the lack of conditioning with practice was caused in part by the number of unreinforced conditioned stimuli presented (the phenomenon of experimental extinction).

References

1. Cason, H. The conditioned pupillary reaction. *J. Exper. Psychol.,* 1922, **5**, 108-146.
2. ———. The conditioned eyelid reaction. *J. Exper. Psychol.,* 1922, **5**, 153-196.
3. English, H. B. Three cases of the "conditioned fear response." *J. Abn. & Soc. Psychol.,* 1929, **24**, 221-225.
4. Fearing, F. The history of the experimental study of the knee jerk. *Amer. J. Psychol.,* 1928, **40**, 92-111.
5. ———. Reflex action. Baltimore, Md.: Williams & Wilkins, 1930.
6. Fulton, J. F. Muscular contraction and the reflex control of movement. Baltimore, Md.: Williams & Wilkins, 1926. Pp. xv+644. London: Baillière, 1927.
7. Hamel, J. A. A study and analysis of the conditioned reflex. *Psychol. Monog.,* 1919, **27**, No. 118. Pp. 65.
8. Hilgard, E. R. Conditioned eyelid reactions to a light stimulus based on the reflex wink to sound. *Psychol. Monog.,* 1931, **41**, No. 184.
9. Humphrey, G. The effect of sequences of indifferent stimuli on a reaction of the conditioned response type. *J. Abn. & Soc. Psychol.,* 1927, **22**, 194-212.
10. Hunter, W. S. Experimental studies in learning. In *The foundations of experimental psychology,* ed. by C. Murchison. Worcester, Mass.: Clark Univ. Press; London: Oxford Univ. Press, 1929. Pp. 564-627.
11. Jones, H. E. Conditioned psychogalvanic responses in infants. *Psychol. Bull.,* 1929, **25**, 183-184.
12. ———. The retention of conditioned emotional reactions in infancy. *J. Genet. Psychol.,* 1931, **37**, 485-499.
13. Jones, M. C. The elimination of children's fears. *J. Exper. Psychol.,* 1924, **7**, 382-390.
14. Lashley, K. S. The human salivary reflex and its use in psychology. *Psychol. Rev.,* 1916, **23**, 446-465.
15. ———. Reflex secretion of the human parotid gland. *J. Exper. Psychol.,* 1916, **1**, 461-494.
16. Mateer, F. Child behavior; a critical and experimental study of young children by the method of conditioned reflexes. Boston: Badger, 1918. Pp. 239.
17. Métalnikov, S., & Chorine, V. Les réflexes conditionnels et la formule leucocytaire. *C. r. Soc. de biol.,* 1929, **100**, 17-19.
18. Moss, F. A. Note on building likes and dislikes in children. *J. Exper. Psychol.,* 1924, **7**, 475-478.
19. Nicolau, I., & Antinescu-Dimitriu, O. Rôle des réflexes conditionnels dans la formation des anticorps. *C. r. Soc. de biol.,* 1929, **102**, 133-134.

20. ————. L'influence des réflexes conditionnels sur l'exsudat peritoneal. *C. r. Soc. de biol.*, 1929, **120**, 144-145.

21. Pavlov, I. Conditioned reflexes: an investigation of physiological activity of the cerebral cortex. (Trans. by G. V. Anrep.) London: Oxford Univ. Press, 1927. Pp. xvi+430.

22. ————. Lectures on conditioned reflexes. (Trans. by W. H. Gantt.) New York: International Publishers, 1928. Pp. 414.

23. Peak, H. Modification of the lid-reflex by voluntarily induced sets *Psychol. Monog.*, 1931, **42**, No. 188.

24. Postle, D. K. Habit formation in the newborn infant. (Dissertation on file in the Ohio State Univ. Library.)

25. Schlosberg, H. A. A study of the conditioned patellar reflex. *J. Exper. Psychol.*, 1928, **11**, 468-494.

26. Switzer, S. A. Backward conditioning of the lid reflex. *J. Exper. Psychol.*, 1930, **13**, 76-97.

27. Watson, J. B. The place of the conditioned reflex in psychology. *Psychol. Rev.*, 1916, **23**, 89-117.

28. Winsor, A. L. Conditions affecting human parotid secretion. *J. Exper. Psychol.*, 1928, **11**, 355-363.

29. ————. Observations on the nature and mechanism of secretory inhibition. *Psychol. Rev.*, 1930, **37**, 399-412.

30. Wolfle, H. M. Time factors in conditioning finger withdrawal. *J. Gen. Psychol.*, 1930, **4**, 372-379.

31. Yarborough, J. V. The influence of the time interval upon the rate of learning in the white rat. *Psychol. Monog.*, 1921, **30**, No. 135. Pp. 52.

University of Illinois
Urbana, Illinois

LE CONDITIONNEMENT COMME FONCTION DE L'INTERVALLE ENTRE LE STIMULUS CONDITIONNÉ ET LE STIMULUS ORIGINAL

(Résumé)

Quand on a conditionné un mouvement d'éloignement du doigt à un stimulus auditif, le nombre des réponses conditionnées a été influencé par les intervalles de temps entre le stimulus conditionné et le stimulus non conditionné. Quand on a présenté premièrement le stimulus non conditionné, le conditionnement s'est montré possible, mais les pourcentages (entre 6 et 12) ont été peu élevés. La relation simultanée n'a pas été la plus puissante, car seulement 10 pour cent de conditionnement a résulté de son usage. Quand on a présenté premièrement le stimulus conditionné, la quantité de conditionnement est restée toujours petite, sauf pour les intervalles entre 0,2 et 0,6 secondes; avec ceux-ci, le pourcentage de conditionnement a été aussi élevé que 58.

L'exercice a eu l'effet de diminuer le nombre des réponses conditionnées qui ont paru, plutôt que de les augmenter. Mais quand on n'a pas présenté le stimulus conditionné non renforcé pendant les trois premières périodes d'exercice, la quantité de conditionnement pendant la quatrième période d'exercice et la cinquième a été beaucoup augmentée. Ce fait indiquerait que la diminution dans le conditionnement avec exercice a été le phénomène de l'extinction expérimentale.

Wolfle

DIE BEDINGUNG (CONDITIONING) ALS FUNKTION DER PAUSE ZWISCHEN DEM BEDINGTEN UND DEM URSPRÜNGLICHEN REIZ

(Referat)

Bei der Bedingung eines Zurückziehens des Fingers nach einem auditiven Reiz wurde die Zahl der bedingten Reaktionen durch die Zwischenzeit zwischen dem bedingten und dem unbedingten Reiz beeinflusst. Wenn zuerst der unbedingte Reiz dargeboten wurde, stellte sich zwar die Bedingung als möglich dar, aber die Prozentzahlen,—die zwischen 6 und 12 schwankten, —waren niedrig. Das Verhältniss der Gleichzeitigkeit war nicht das wirksamste, da bei Gebrauch dieses Verhältnisses in nur 10% der Fälle Bedingung stattfand. Wenn zuerst der bedingte Reiz dargeboten wurde, war der Betrag an Bedingung (amount of conditioning) noch immer gering, ausser bei Zwischenzeiten von 0.2 bis 0.6 Sekunden. Bei diesen ragte der Prozent der Bedingtheit bis zu 58.

Die Einwirkung der Einübung verminderte eher die Zahl der stattfindenden bedingten Reaktionen anstatt sie zu vergrössern. Wenn aber der unverstärkte bedingte Reiz nicht während der drei ersten Übungsperioden dargeboten wurde, so wurde der Betrag an Bedingung während der vierten und fünften Übungsperiode stark vermehrt. Diese Tatsche scheint anzudeuten, dass die Verminderung der Bedingung im Laufe der Einübung das Phänomen des experimentellen Aussterbens darstellt.

WOLFLE

SIMPLE CONDITIONING AS A METHOD OF STUDYING SENSORY DISCRIMINATION AMONG IDIOTS*[1]

From the Training School at Vineland, New Jersey

EDGAR A. DOLL AND CECELIA G. ALDRICH

I

GENERAL PROBLEM

Idiocy is that simple form of human evolution in which intellectual and social development remain at the infant level while physical development approaches normal age standards. The idiot thus furnishes a slow-motion picture of mental growth. His genetic development and his level of adaptive behavior resemble those of normal human infants and of adult anthropoids (2, 3, 4), but his mental development is so nearly completely arrested that his behavior may be studied relatively independently of the factor of growth increments. The fact that idiocy is, in certain respects, a condition of permanent infancy, makes possible important researches in human psychology, if sufficiently simple and precise methods of investigation can be devised.

The conditioned reflex, as a scientific method, has opened many new fields of investigation, proving especially valuable in studying the simpler forms of human behavior and making possible a new experimental psychology of infants. In this study we have undertaken to adapt the method of simple conditioning for the experimental investigation of sensory discrimination with idiot subjects.

The term "conditioning," as used in this study, refers to the principle that if a given stimulus which habitually brings forth a characteristic response, owing either to inherited mechanism or previous association, occurs simultaneously and repeatedly with a second stimulus,[1a] the second stimulus will thereafter bring forth the response previously associated only with the first.

---

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, November 5, 1931.

[1] This is a report of one of a series of psychological studies of idiocy, conducted at The Training School at Vineland under a financial grant from the Elmhirst Fund.

[1a] The simultaneity may be only approximate and the repetitions few or many.

Extensive experiments in conditioning have been performed with animals, principally dogs, whose salivary reflexes and other involuntary activities have been modified to respond to initially irrelevant stimuli by a process of associative substitution. There has also been some previous work with both normal and defective children. Krasnogorski (13) was one of the first to conduct such experiments. Using food as the unconditioned stimulus, he conditioned blindfolded children to opening their mouths when various parts of their bodies were stimulated. Later, Mateer (14), using an adaptation of Krasnogorski's method, established an association between placing the blindfold and opening the mouth. Mateer studied not only the process of conditioning, but also the reconditioning and inhibiting of the responses learned. The principal data collected by both Mateer and Krasnogorski were obtained with normal children, although there was some preliminary experimental work with mental defectives.

In the work with defectives, Mateer concluded (p. 181) that "the reaction is nearer that which one would expect of their chronological age than that which corresponds to the same mental age of our unselected group." This conclusion was reached in spite of the fact that in some cases there were wide discrepancies between the mental ages and life ages of the subjects. With respect to the conditioning reflex mechanism as a whole, Mateer stated (p. 191): "If a process is fundamentally necessary for normal functioning there should in all probability be greater deviations from the norm among defectives than in the unselected group."

Burnham (6) devotes several chapters to a discussion of conditioning among both normal and feebleminded children, and concludes that the ability to form conditioned reflexes develops earlier in some children than in others, and earlier in the normal than in the feebleminded. He adds, more specifically: "Not until in the course of the second year does the mechanism of the conditioned reflex attain its complete development and functional perfection." With respect to delayed response or the formation of a memory reflex, Burnham concludes that this is easier for normal children than for animals, but difficult for the imbecile and impossible for the idiot.

These experimental studies suggest the importance of conditioning as a method of training. They have also contributed a great deal to the revision of child guidance technique, especially for the preschool years. Cowan and Carlson (7) present the conditioning method as it may be applied to behavior problems and interpreted in lay style for parents.

Aside from academic considerations, there is the practical problem of the idiot himself. Surveys of idiot capabilities (9) and reports of intensive training (17) suggest that idiots can be raised beyond the level of self-help to that of constructive activity. Intensive studies of idiots should bring to light these capabilities and suggest new methods of training.

The Method

The development of a method suited to these ends was a serious problem. Our results in that direction are probably more important than the data obtained by the use of this method. In order to study conditioning, it was necessary that there be an unconditioned or previously conditioned stimulus which would habitually bring forth a given response and, by associating that response with the new stimulus, build up a response for the latter. It was desirable that the procedure should be as objective as possible and that the set-up be adaptable for comparative studies with animals and preschool children.

Previous experimental work (3, 4) had indicated that problem-solving takes place spontaneously when there is a desired objective to be obtained and no observers to be relied upon for help or cues. Therefore, in the present problem, we dispensed with verbal stimulation, used a food lure, and eliminated the observer from the experimental room. After many preliminary attempts, the following satisfactory set-up was evolved. The children adjusted, not only readily, but eagerly.

An experimental room (Figure 1), which was used exclusively for this purpose, was located in the cottage in which the subjects lived. The room was divided into experimental and observational sections by a partition, equipped with a one-way vision screen 8 inches wide and 4 feet above the floor. The window and radiator, which were located at one end of the room, were excluded by a second partition, leaving the experimental section a symmetrical room 10 by 14 feet. This room was illuminated by a diffuse ceiling light, while the observational section was dark except for a well-shaded light on the recording shelf. This almost total darkness in the observation room increased the efficiency of the one-way vision screen.

The entrance to the experimental section was formed by two 5-foot

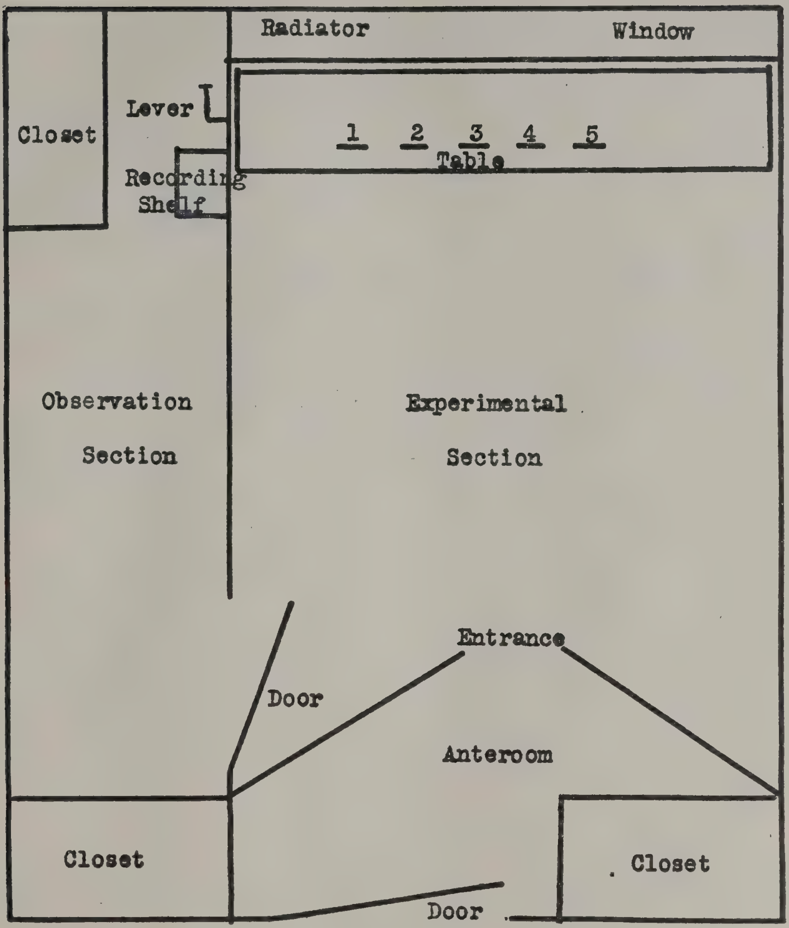

Radiator
Window
Closet
Lever
1
2
3
4
5
Table
Recording Shelf
Observation Section
Experimental Section
Entrance
Door
Anteroom
Closet
Closet
Door

FIGURE 1
Diagram of Experimental Set-Up

partitions, which extended out from the corners of the room, allowing a 26-inch opening between them. These formed an anteroom between the door of the room and the actual entrance to the experimental section.

A table, 24 inches wide and 20 inches high, extended across the full width of the experimental room, at the end opposite the door.

On the table were two cubic boxes, identical in every respect except size. Five boxes were used, in pairs, ranging in size from 9 to 16 cm. on a side. They were made of quarter-inch pine, painted a light gray. These boxes were hinged to the table so that they could be raised in only one direction, i.e., away from the child as he approached the table. A metal tongue on the inside of the face of each box fitted through a hole in the table, making it possible to lock the boxes by turning a lever at the recording shelf. The boxes could be placed in any of five positions, 10 inches apart. This facilitated changes in their relative positions.

A food lure was placed under the box to which the subject was to be conditioned. The incentive value of such a lure had been demonstrated in previous experimental work with idiots (1).

The observer was not seen by the child while the experiment was in progress. However, the personal element was not entirely eliminated, since all the children associated the observer with the situation and, when they saw her at other times during the day, frequently asked to be taken to the experimental room.

An assistant, a 16-year-old boy with a mental age of six years, brought each subject to the room for each trial and, at the sound of a buzzer, admitted him to the anteroom. The assistant remained outside until the buzzer was again sounded, signifying the completion of the trial, when he opened the door and returned the subject to the playroom. Time was kept by the observer, who sounded the buzzer when the trial was completed. During the experimental situation as a whole the observer remained behind the one-way vision screen.

Each child was given five trials a day, at 10-minute intervals, during which the subject was returned to the regular playroom. The trials occurred between 8:30 and 10:00 A.M., seven days a week, with exceptions only when necessary because of illness or unavoidable vacations on the part of the subjects.

The boxes were located in Positions 2 and 4, referred to throughout this study as left and right, respectively. The actual position (left or right) of each box in a series of 100 trials was determined in advance by tossing a coin.

The time interval between the child's appearance at the entrance and his raising of the box was recorded in seconds. After the choice had been made, the observer locked the remaining box and sounded the buzzer, as a signal for the assistant to open the door and return the subject to the playroom.

Since a period of adjustment seemed necessary, five preliminary trials were given to condition the subjects to the situation as a whole. During these trials there was only one box in the room. This was placed in Position 3, in the center of the table. Arnold, who was eliminated from the study because of an extended illness, presented a typical example of initial adjustment among the slower members of the group. The following account describes his behavior during these first five trials.

> *Trial 1.* Arnold stood at the door 90 seconds, trying to open it, then walked through the entrance to the center of the room, crying softly. He went to the table and poked his fingers into the vacant holes made for the metal tongues on the boxes. He rubbed his hand over the box and finally tipped it back, took the cookie and ate it. The buzzer sounded and the assistant called, but Arnold did not leave the box until the assistant came in and took him by the hand, leading him out. Time: 135 seconds.
>
> *Trial 2.* Arnold went to the right wall of the room and rubbed his hand along it to the table. He ran his finger along the edge of the table to the box, raised the box and took the cookie. The buzzer sounded and Arnold went to the door when the assistant opened it and called him. Time: 32 seconds.
>
> *Trial 3.* Arnold went to the partition and tried the door to the observation room, then proceeded directly to the table and raised the box. He went to the exit door when the buzzer sounded. Time: 9 seconds.
>
> *Trial 4.* Arnold went to the table immediately, took the cookie and left. Time: 5 seconds.
>
> *Trial 5.* Arnold ran to the box, took the cookie and ran back to the door. Time: 3 seconds.

During these trials for initial adjustment, the subjects became conditioned to the various aspects of the experiment. The buzzer to leave the room was one of the conditioned stimuli to which they reacted. The following examples are illustrative of such conditioning and show how quickly the children became adjusted in a situation devoid of verbal instructions.

> George usually started to the door immediately after making his choice, then stopped abruptly and waited in the center of the room until he heard the buzzer.
>
> Walter stood at the table, with the box lifted, and made short, quick vocalizations which soon came to mean that he had made his choice and was ready. As soon as the buzzer sounded, he slammed the box into place and left the room.

Dan, on several occasions, started to make a second choice but stopped abruptly and left the room when the buzzer sounded.

Teddy, on a few occasions, became playful and jumped about the room, ignoring the boxes. However, when the buzzer sounded for him to leave, he ran to the boxes immediately, to get the cookie before being ushered out.

THE SUBJECTS

The subjects of this study were ten feebleminded children who, on January 1, 1931, ranged in mental age from 1.5 to 3.3 years (as measured by the Kuhlmann-Binet Scale[2]), and in chronological age from 3 to 12 years. The children are referred to throughout the study by the names and numbers assigned to them in Table 1, in which they are listed in the order of their mental ages. Life age, IQ, and standing height are also presented in this table.

TABLE 1
DESCRIPTION OF SUBJECTS

No.	Name	Kuhlmann MA in months	LA in years	IQ	Standing height Cm.	Av. age in years*
1	Jimmy	39	12	28	1280	9.5
2	Carl	38	10	33	1205	7.5
3	Dan	36	9	34	1275	9.5
4	Dick	36	7	41	1200	7.5
5	Bobby	34	8	38	1100	6.0
6	Walter	33	9	30	1232	8.5
7	Sammy	30	10	26	1310	10.0
8	Tim	28	12	20	1360	11.0
9	George	18	12	14	1320	10.5
10	Teddy	17	3	48	1050	5.0

*The average age for standing height is computed from the Smedley norms of physical development.

A brief descriptive paragraph on each subject is given, to supplement the data in Table 1. These data take into account etiological factors as far as known, physical differences, capabilities, and a few typical personality reactions. Complete case histories are on file at the Vineland Laboratory.

---

[2] In our study of comparative genetic intelligence of idiot and preschool children, we found that the Kuhlmann-Binet was probably the best general estimate of the child's intelligence, although it underrated his manual ability. This is also the most widely used and most generally accepted test in these lower levels. Later, under the discussion of mental age and learning ability, other test scores will be considered.

Jimmy was of the Mongolian type, physically under-developed, and frequently subject to coryza. He had the highest test age of the group, and apparently functioned socially at that level. He was helpful in regular cottage life, where he frequently folded and put away clothing not being used. He could dress and undress himself and often helped with the other children. Eager for attention, he was of the "show-off" type.

Carl was from a family with neuropathic tendencies but average intelligence. He was physically under-developed and was subject to a chronic catarrhal condition. He attended school regularly, but training made little impression on him, although he had the second highest test age of the group. His behavior was destructive and he frequently pilfered toys and articles of clothing. He had a surly disposition and a swaggering walk which was incongruous with his slight stature. He was independent and disobedient.

Dan was a frail but very attractive child, usually quiet and inactive. There was no history of abnormal heredity, injury, or disease, but his general reactions suggested a constitutional instability. Occasionally he wandered about the room, humming kindergarten tunes, or surprised an observer by counting articles or naming colors. His usual verbal response, however, was merely to repeat words spoken to him.

Dick was of the Mongolian type, very neat in appearance and of a happy disposition. He attended kindergarten and learned to do elementary manual tasks. He expressed delight at his own successful responses and sometimes showed disappointment when unsuccessful. He was eager and responsive, if interested, although somewhat shy.

Bobby was a short, stocky, broad-shouldered little boy, also of the Mongolian type, with a verbal ability and alert attitude that gave an impression of superficial brightness. At times he was shy and stubborn.

Walter, another Mongol, was awkward and unattractive in appearance, but soon won friends by his amusing antics. Like Carl, he responded only slightly to training. He seemed extremely self-conscious when attention was given him, and responded by hanging his head or closing his eyes tightly and grimacing. He talked in monosyllables, in a deep, gruff voice.

Sammy's condition was supposedly of traumatic origin, due to a fall at two years of age. Up to that time his developmental history had been, apparently, normal, whereas shortly afterward mental growth was arrested. He was an attractive little boy, and appeared alert and responsive to training, although his behavior manifested little if any evidence that such was the case. He was extremely active and mischievous.

Tim was a well-developed child with a mild thyroid deficiency. His history revealed no assignable cause for his condition. He was apparently quite unresponsive to training, even in habits of self-help, and frequently annoyed, pushed, or slapped the other children, tore books and broke toys, apparently to attract adult attention. Such obstreperous behavior often followed repeated failure in the experimental room.

George had a history of difficult birth and, at the time of this study, was somewhat anemic. He limped on the left foot. He was a hyperactive little boy, always eager to do what was expected of him. If another child was told to take off his hat and coat, George immediately proceeded to take it off for him. His eagerness, however, often resulted in poorly directed activity and seldom amounted to constructive aid, either in helping himself or others.

Teddy was a beautiful child of three years, physically well developed, who had been diagnosed as probably epileptic. He was active and could respond well in some lines of self-help, but had not proved trainable in all. The most obvious retardation was the complete absence of language. He was mischievous and affectionate, but generally unresponsive to the normal interests of a child of his level. His attention was exceedingly difficult to sustain.

It will be noted, from the table and descriptive paragraphs, that most of these children had specific physical as well as developmental defects and that four subjects were of the Mongolian type. However, there were few, if any, personality defects among these children as a group that are not found in different degrees among normal preschool children.

Although this may seem to be a mixed group, representative of various developmental defects, rather than simply a group of idiots, it appears likely that such findings are more or less typical among low-grade feebleminded children. Goddard's (10) conclusion that, other things being equal, the mentality of the child will approximate that of the parents, cannot adequately be applied to children of idiot grade. It is, therefore, reasonable to expect, as Tredgold (16) suggests, a neuropathic diathesis rather than direct heredity. In this case there would naturally be more divergence from simple subnormality and the idiots would, in many cases, be "subnormal plus."

Subnormality of development, however, is their chief divergence from the normal, and this subnormality is primarily mental. Therefore, despite complicating factors, we have human intelligence of an elementary form, and comparative studies will show its significant differences from normal intelligence of approximately the same level. The method established for this study can be readily adapted for such comparative purposes.

II

Conditioning and Reconditioning Experiments

The Problems. In the first phase of the study we were interested in (*a*) factors influencing conditioning and (*b*) the relation of conditioning to reconditioning or unconditioning.

There were four problems, in all of which the two boxes, 12 and 16 cm., respectively, were used. In Problem I, the subjects were conditioned to the 12-cm. box; in Problem II they were conditioned to the 16-cm. box. Thereafter, the preferred box alternated, so that in Problem III it was the 12-cm. box and in Problem IV the 16-cm. box. The problems were presented in numerical order. The last three problems involved inhibition, inasmuch as reconditioning includes the inhibition of a learned response as well as the formation of another.

Three subjects failed the first problem and, with them, changes in incentive were employed as a means of facilitating learning by increasing the desirability of the correct choice. These changes, as described later, were used when there had been no apparent alteration in the subjects' status for a large number of trials.

A second change from the usual technique was effected by placing the correct box always on one side. This was used as a last resort in overcoming position habits and was tried with only one subject.

There were other changes in technique, such as enforced intervals due to illness or unavoidable vacations of the subject. The effect of all of these changes is discussed later.

Results. The results in so small a group naturally indicated marked individual differences. Analysis was directed, therefore, toward an evaluation of these differences and the factors which accompanied them. There were some general tendencies, however, which somewhat discounted the importance of individual differences.

Mateer (14), finding similar results, concluded: "Differences which remain when age and sex have been evaluated, can hardly be attributed to 'just individual differences' any more logically than they can to chance errors of experimentation, unless we find the same variation in other corroboratory findings and these cannot be picked up by chance afterwards, but must be a part of the original data." We, therefore, attempted to record all factors which in any way accompanied the experimentation, in order that they might be evaluated together with the mental and life ages of the subjects.

The original observations included the name of the subject, position of correct choice, notes on activity prior to choice, the

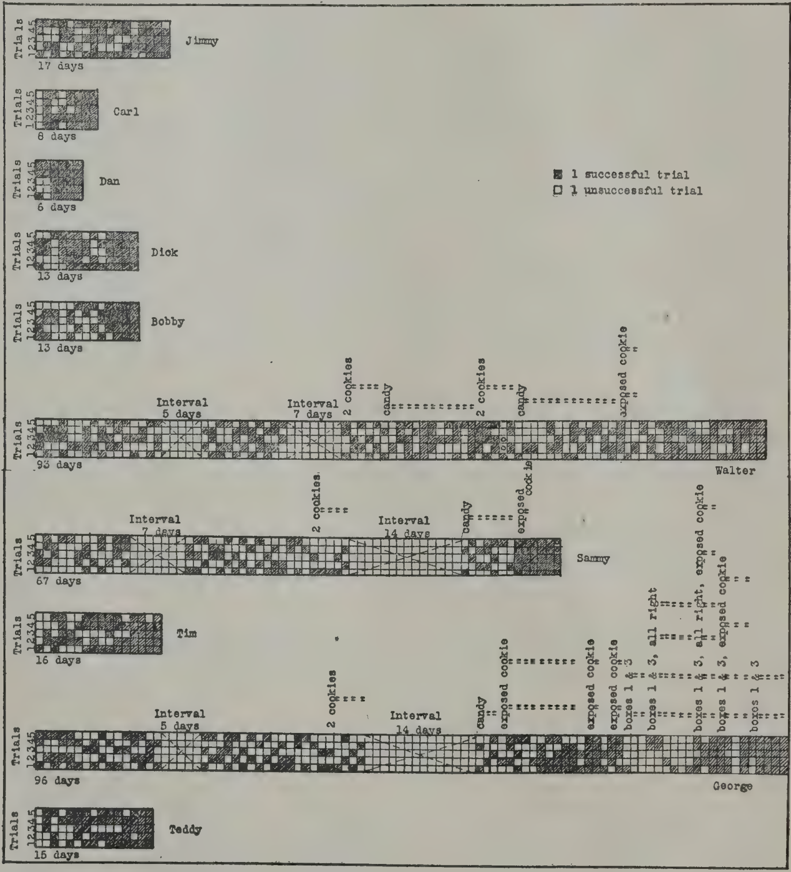


FIGURE 2
SHOWING TRIALS PASSED AND FAILED ON EACH DAY OF LEARNING PERIOD IN PROBLEM I

choice made, the time taken, and notes on any change from the regular technique. These original data are summarized in Figures 2, 3, 4, and 5.

These figures show the trials passed and failed during each day of the learning period, and make it possible to study the rate at which learning took place. These graphic representations display the differences in learning between Problem I and those following, the

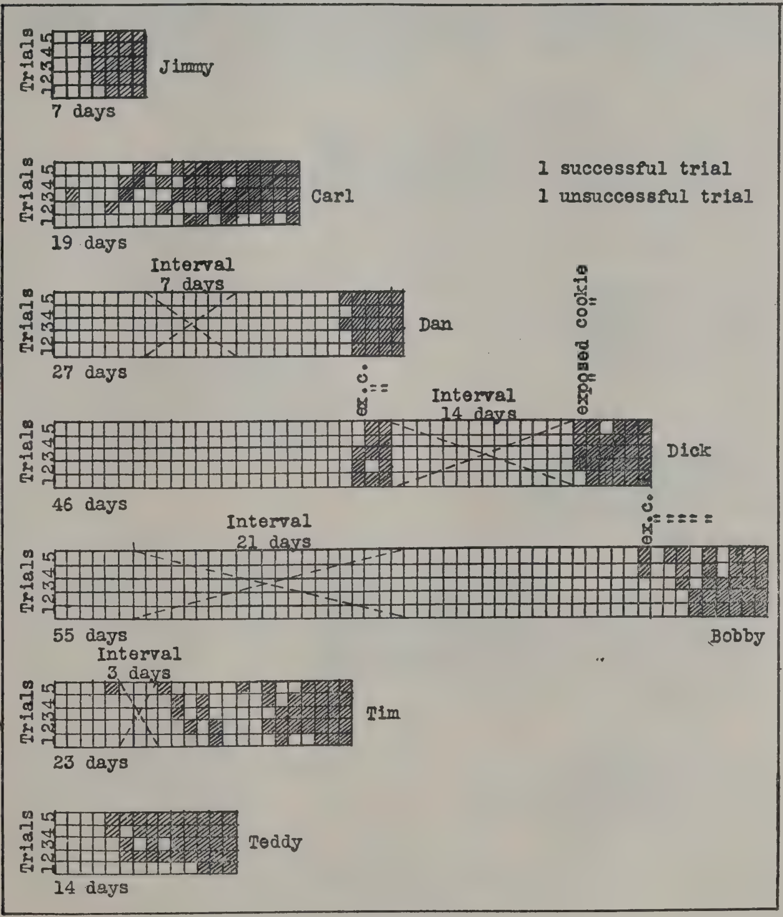


FIGURE 3
Showing Trials Passed and Failed on Each Day of Learning Period in Problem II

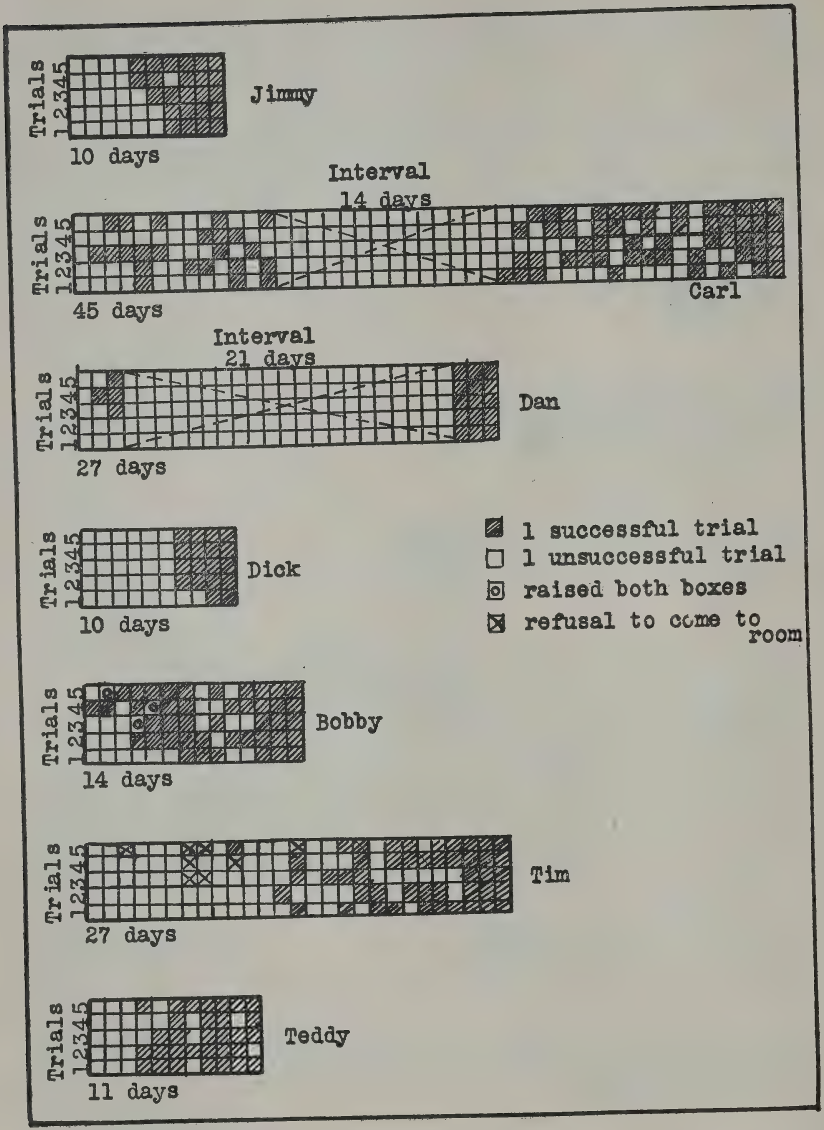


FIGURE 4
SHOWING TRIALS PASSED AND FAILED ON EACH DAY OF LEARNING PERIOD IN PROBLEM III

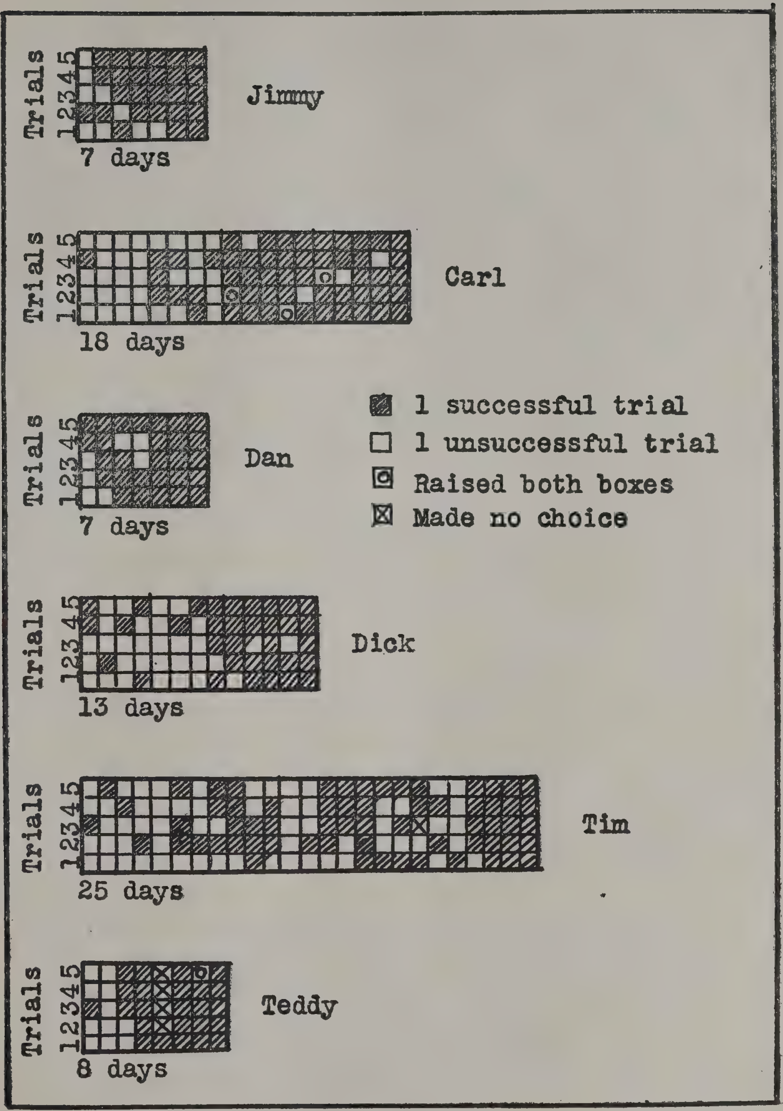


FIGURE 5

Showing Trials Passed and Failed on Each Day of Learning Period in Problem IV effect of the incentive on each trial, and the perseveration of a learned response. The blank spaces represent failure, and the solid spaces, success. Other responses, such as raising both boxes or making no choice, are represented on each figure. Intervals are marked out by crossed lines to avoid confusion with days on which there was consistent failure. Any change from the usual lure is marked opposite the day on which it occurred, and the omission of a mark means that the original lure, a cookie under one box, was used. Other changes in technique are recorded in like manner.

These graphic results are supplemented by Table 2, which summarizes the quantitative data in terms of the total number of trials taken by each subject on each problem.

TABLE 2
NUMBER OF TRIALS TAKEN BY EACH SUBJECT ON EACH PROBLEM

No.	Name	Problem I	Problem II	Problem III	Problem IV
1	Jimmy	85	35	50	35
2	Carl	45	95	155	90
3	Dan	30	100	30	35
4	Dick	65	160*	50	65
5	Bobby	65	170*	70	—
6	Walter	405*	—	—	—
7	Sammy	230*	—	—	—
8	Tim	80	100	135	125
9	George	385*	—	—	—
10	Teddy	75	70	55	40

* Not learned until after changes in incentive.

For clarity of discussion, the separate phases and influential factors are considered individually and reference is made to the tables and graphs throughout. These are further supplemented by additional tables, pertinent to the discussion at hand.

Discussion

General features. Consideration of the results obtained when idiot children are presented with simple learning problems leads to many inferences with respect to the learning process itself and to numerous suggestions for more extensive research.

Only seven of the ten subjects learned the first problem. The three who failed contributed markedly to experimentation on changes in lure and technique, the results of which are extremely suggestive.

Among some subjects there was marked perseveration (as defined later) on the last three problems, this perseverative tendency being greatest on Problem II. The total number of trials required on Problem II for most subjects exceeded that of any other problem, although two subjects reduced the required number on each problem from the first to the fourth. The effect of intervals on learning status seemed negligible.

On Problem I, every subject revealed a position preference, choosing always on the left or always on the right for at least a day, regardless of which box was in that position. The three strongest position habits were formed by the children who failed Problem I. No position habits were formed in any other problem.

In each table the subjects are listed in order of Kuhlmann-Binet mental age. It is obvious that within these mental age ranges there was no marked relation between learning and performance, although the general tendency was for those of higher mental age to learn more readily. Life age showed apparently no relation to learning, even though the differences in life age were great.

Conditioning on consecutive problems. Referring to Table 2, it is apparent that those subjects who completed all four problems did as well or better on the fourth as compared with the first problem. This was true in spite of the fact that the two problems were of the same order, the major difference being the experience of the child at the time of their presentation. There was apparently some degree of transfer of the identical elements in the situations, which enabled the child to reduce his learning period by the time the fourth problem was presented. This, however, was not true from the first to the second problem, in which the number of trials required was markedly increased for all but two subjects. The tendency of the group, therefore, was to increase the number of trials from the first to the second problems but to decrease the number of trials taken on Problem IV to equal or below that of Problem I.

Although this was the group tendency, it was by no means without exception. In fact, the subjects can be divided into three groups, according to the increase or decrease in number of trials from problem to problem. This is shown graphically in Figure 6. The first three (Group I) show decided increase on Problem II, followed by a drop. The fourth and fifth (Group II) show a decrease from the first through the fourth problem. The sixth and seventh (Group III) show a steady increase to Problem III, but a drop in Problem IV.

From these results it seems plausible to conclude that a general factor of learning operated throughout the problems, although the full benefit was not derived immediately because of the interference of specific conditioning. After some experience, the amount of which varied with the individual, the subjects seemed to grasp the "general situation." In fact, two subjects (Jimmy and Dan) were given six problems and became so adept that one choice without reward was sufficient to effect a change in choice.

Perseveration. The term "perseveration" has been applied to the tendency to persist in a given type of response. This tendency for behavior processes to have a certain lag has been recognized for some time, and various attempts to measure it have been undertaken. Such attempts, as reviewed by Jasper (11), "fail to support the hypothesis of a broad factor of perseveration." However, he quotes Spearman as saying that "there does appear to exist a unitarily functioning factor varying in degree from one individual to another, a tendency for mental processes to have a certain lag or inertia and in this meaning to 'perseverate.' "

This specific type of perseverative tendency is the type defined by Cushing (8) as "the tendency to continue a given line of activity after the incentive for such activity has been reduced to a minimum," and is the type of perseveration which we were able to observe in varying degrees among our subjects.

In Problem II, after the lure had been removed from the smaller box and placed under the larger, there was a tendency among the subjects to continue to choose the smaller box, despite the fact that the incentive responsible for the establishment of this choice had been removed. As recorded in Table 3, all of the seven subjects given Problem II revealed this tendency for more than five trials, or one

TABLE 3

NUMBER OF TRIALS IN WHICH SUBJECT CONTINUED TO REVEAL A PERSEVERATIVE TENDENCY OR TO MAKE THE PREVIOUSLY CONDITIONED CHOICE WITHOUT REWARD

No.	Name	Problem II Trials	Problem III Trials	Problem IV Trials
1	Jimmy	14	23	1
2	Carl	7	7	3
3	Dan	82	8	3
4	Dick	115	31	3
5	Bobby	123	3	
8	Tim	24	42	2
10	Teddy	23	15	2
	Total	388	129	14

day's performances. This numerical representation is taken from Figures 3, 4, and 5.

Five subjects (Figure 3) changed spontaneously to the opposite box after from 7 to 82 trials, while two (Dick and Bobby) did not show any evidence of spontaneous change. After more than 100 unsuccessful trials by these two subjects, a cookie was placed *on* as well as *under* the opposite box. Even then these subjects continued to choose the originally conditioned box occasionally for about 25 trials, after which they consistently chose the larger box, on which the cookie was placed. When the cookie was removed from the top of the box they continued to make the newly conditioned choice.

Perseveration from the second to the third problem was much less than from the first to the second. The seven subjects ranged from 7 to 49 as compared with from 7 to 82 in the number of trials taken before trying the opposite box. With the exception of the one child who took 49 trials for perseveration on Problem III, there was a decrease in the perseverative tendency from problem to problem.

These results account, in some measure, for the increase in the number of trials between Problem I and Problem II and the decrease thereafter, inasmuch as the perseverative tendency follows the same general trends. However, a few subjects increased the total number of trials required far more than could be accounted for on the basis of absolute perseveration. Such increases might still be attributed to the interference of the previous conditioning. This interference was evident during the change from wrong to right choices or during the transition period, which varied among individual subjects, some changing abruptly, as did Dan and Jimmy, others continuing to make one wrong choice each day and others increasing the correct trials gradually, making one or two per day, then three or four, and finally five.

Problem IV revealed the least perseverative tendency. In fact, no subject made the immediately preceding choice for all of the first five trials (see Table 3).

Individual differences in relation to trainability. These variations in perseverative tendency suggested that, with respect to ultimate trainability, the ability displayed on the first problem was not so important as the ability to transfer training from one step to the next. According to the results on Problem IV, perseveration followed by abrupt change was more indicative of trainability than was little perseveration followed by a long transition period, although

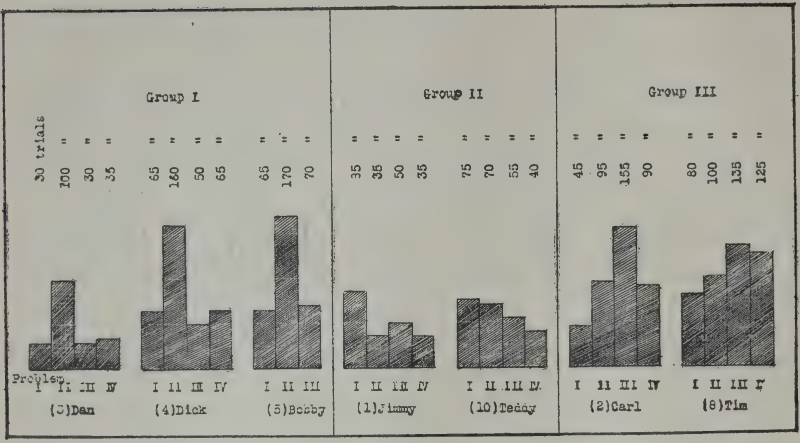


FIGURE 6

Graphic Representation of Number of Trials Required on Each Problem
Subjects are grouped according to tendencies to increase or decrease their number of trials from problem to problem.

the former apparently lengthened the time required for the immediate problem.

According to results on the last problem, the two subjects (Carl and Tim, who constitute Group III of Figure 6) who increased the number of trials necessary in successive problems and showed only a minor degree of perseveration might be expected to be the least trainable. An investigation of their response to cottage and kindergarten training confirmed this expectation. The three (Dan, Dick, and Bobby, who constitute Group I of Figure 6) who learned in less than 70 trials on the first problem, and then persisted in the established choice, might be expected to profit most from training in routine tasks requiring constant repetition. Dan, who spontaneously broke down his persistence and readily adapted on the third and fourth problems, might be considered more trainable than Dick and Bobby, for whom the established choice persisted until additional stimulation was given. Jimmy and Teddy (Group II of Figure 6) might be expected to be most adept in learning new and varied tasks, inasmuch as they showed the greatest transfer from one problem to another. Jimmy confirmed this expectation in his helpfulness about the cottage. Teddy also displayed some adaptability but at a different level, including such items as attempts to feed and dress himself.

Position preferences. Some of the subjects showed a strong tendency to choose a box first according to position, although later establishing the preference on the basis of size. On Problem I, all but two of the subjects chose one position consistently for periods of from 2 to 30 days. Four of these preferences were for right and four for left, with no apparent relation between such preferences and the "handedness" of the child. The extreme cases of position preference were among the three (Walter, Sammy, and George) who failed the first problem until it was modified. Among these three a change in incentive was necessary before the position preferences were permanently overcome.

Bobby's position preference suddenly gave way to absolute learning. There was no intervening period between the breaking of his position preference and the establishment of the correct choice.

Jimmy's position habit changed from left to right. This was followed by a period of random choice before learning.

In five subjects position preferences were eliminated after 10 or 15 trials, and thereafter correct choices were increased until the problem was learned.

By the constant choice of one position, the subject would obtain the lure approximately 50 per cent of the time in the course of 25 trials. This permitted a maximum of five correct choices a day, without any discrimination on the basis of size.

According to the chance series which determined the position of the boxes, groups of five consecutive placements on one side occurred during the third and fourth days of the learning period. It was at this time that Jimmy's position preference shifted, and those of five other subjects were eliminated. It thus appeared that Bobby's was the only case in which the preference was sufficiently well established to be continued in the face of continuous failure. The position preferences of the three unsuccessful subjects were later re-established, although they were overcome temporarily at this early period by repeated failure on one day. In two of these three cases (Walter and George) the preferences were suspended for four days and then re-established to last until changes in incentive occurred. In the case of Sammy, the third member of this trio, the position preference shifted from the right to the left position.

Later in the learning period, position habits among these three subjects were not even temporarily eliminated by failure for five consecutive trials, nor were they influenced by periods of absence.

In fact, changes in incentive, as discussed later, were the only means of breaking these preferences, which had become habitual, and of substituting a discriminatory choice rather than a position choice.

Problem I was the only problem during which position preferences occurred. In later problems, as previously mentioned, the choice was more often influenced by perseveration than by position preferences.

From these results we might conclude (*a*) that there was a tendency for position preferences to become established, at least temporarily, before discrimination on the basis of size took place, (*b*) that these position preferences were evenly divided between right and left, with no marked relation to the handedness of the child, and (*c*) that this tendency was more persistent, with only one exception, among those children who failed the problem.

Changes in incentive and technique. As previously mentioned, an earlier study (1) suggested the importance of incentive in problem-solving among idiot subjects. We wished to investigate this further in simple learning problems and considered the three children who failed Problem I as excellent subjects for the investigation.

The incentive could be considered neither adequate nor inadequate if the child learned the problem under the original circumstances, as seven of the subjects did. Had the incentive been increased, they might have learned much faster; had it been decreased, they might have learned with the same efficiency. These possibilities could not be studied without a control group.

However, for the three subjects who failed the problem, the lure was changed quantitatively and qualitatively as well as in manner of presentation.

The first change was made quantitatively; instead of one cookie under the correct box, there were two cookies. The second change was qualitative; candy was substituted for the cookie. The third change involved a number of variables; the cookie was used as before, except that half of it was placed on top of the correct box and the other half under the box. This was designed to attract the attention of the children to the correct box. After accomplishing this result to the extent that the box on which the cookie was placed was chosen, the exposed cookie was no longer used, and the problem was presented in its original form. If the choice had been made merely by identification with the cookie, the problem could not have been solved when the cookie was removed, but if such identification had carried over to the size of the box, then solution was possible. The same criterion of learning prevailed here as before, that is, 18 correct out of 20 trials on the problem in its original form of presentation.

With the quantitative change, Walter's position preference shifted from the right to the left. George and Sammy, however, displayed no change in overt behavior. Both continued to make left-hand choices, regardless of the box in that position.

The change from a cookie to candy was accompanied by an increase in the number of correct responses made by Walter,[3] whose position preferences were disrupted to the extent that he made from one to five correct responses a day, although he never reached the criterion of learning (18 out of 20 responses correct). A return to the cookie lure was accompanied by his resumption of a position habit.

Sammy's position preference continued with the candy lure as well as with the cookie lure. The only overt evidence of change was his apparent delight upon discovery of the candy lure.

George completely rejected the candy. During the first few trials he tasted and replaced it, but after the fifth trial he merely reclosed the box, as if there were no lure present.

The number of trials given with these lures varied according to their effectiveness. George was given candy only three days (Figure 2), inasmuch as, for him, candy was apparently a neutral stimulus. Sammy was given the double cookie lure and the candy lure seven days each, to make sure that his position habit could not be broken by this method. Walter displayed evidence of learning, and the candy lure was therefore continued for twelve days, following which a return to the cookie lure resulted as noted above.

After these varied periods, each of the three subjects was tried with an exposed lure, the effect of which varied considerably. Sammy immediately chose the small box, on which the cookie lay, taking it and raising the box. After ten such trials the exposed cookie was removed and he continued making the correct choice, which seemed to indicate that association between the cookie and the small box had been established.

---

[3] With a position preference, the lure would be discovered in half of the trials because of the chance arrangement. Hence, the changes in lure would soon be evident to the subject, even if he made no "correct" choices except by chance.

George took the cookie from the right-hand box and started away. However, when the buzzer for exit did not sound, he returned and raised the box in the left position. George always ate the exposed cookie before looking for more, and it was only after 50 trials that he consistently looked for the second piece under the box on which the exposed cookie had been placed. Even then, with the removal of the exposed cookie, his old position preference returned. It was not until the difference between the boxes had been increased, the correct box placed on the right for 50 consecutive trials, with the visible lure upon it, that George's position preference for the left was finally overcome and discrimination became evident. It might be added that no one of these methods alone was adequate, although later results on size discrimination showed that increasing the difference in size might have been unnecessary. The exposed lure, together with placing all on the right, seemed most effective in breaking this position preference.

Walter, it will be remembered, displayed a favorable reaction to candy as compared to a cookie lure. Therefore, the hidden lure was candy and the exposed lure a cookie. Walter took the cookie in every trial, but after the first day consistently raised the opposite box. When the exposed lure was removed, his percentage of correct choices increased and finally he learned the problem, with candy as a lure.

These results give some interesting suggestions as to the effect of incentive on performance. No general conclusions can be drawn because of the small number of instances, and the fact that the conditions were more or less adapted to the child. However, there is confirmation of the results of the previous work (1).

Walter's responses increased in effectiveness with a qualitative increase in the lure, just as some of the subjects of the previous study displayed unsuspected abilities following a similar increase in lure.

There was a suggestion of a neutral incentive when George rejected the candy. This might have been followed up by using two lures simultaneously, one neutral and one positive, to determine whether or not discrimination would be in accordance with the positive lure.

The exposed lure served the purpose of attracting the attention of the subjects to the correct box. The identification of the choice with the lure was established before discrimination was brought about. George, for example, could consistently choose the correct box as long as the lure was on the box, but with its disappearance he failed to carry over the identification to the size of the box.

This same method of placing a cookie on the correct box was used and found effective in breaking down the perseverative tendencies which in the cases of Dick and Bobby, after 100 trials, had shown no evidence of being spontaneously overcome.

There was no reason to suspect that the lure was in any case a negative stimulus, leading to consistent choice opposite to that intended.

Learning and mental age. Learning, as measured by the number of trials taken on these problems, was expected to show some relation to mental age as measured by the Kuhlmann-Binet Scale. This expectation was only partially fulfilled. Seven of the ten subjects completed the first problem in from 30 to 85 trials. Although the three who failed (Walter, Sammy, and George) were in the lower half of the group, they were not the lowest, according to these test scores.

However, with the exception of Jimmy and Teddy, the seven successful subjects showed a perfect relation between learning on the first problem and mental age. Jimmy, who had the highest mental age, required the greatest number of trials. Teddy, whose mental age was the lowest of the entire group, surpassed Jimmy and the three children who failed, all of whose mental ages exceeded his. Such exceptions in a group of this size indicate that other factors than mental age (as this term is usually employed) are operative in learning of this type.

Problems II, III, and IV tended to increase the discrepancy between Kuhlmann-Binet mental age and learning ability. The results on Problem IV, which involved a combination of native ability, experience, and general adaptability, can probably be taken as the best indication of the relative ability of these particular subjects. Three of the subjects finished Problem I only after additional stimulation, and were not given problems beyond that. Bobby completed only three problems, due to numerous absences. The remaining six subjects required between 35 and 125 trials in order to complete Problem IV. The fact that this variation is greater than that found on Problem I among the same six subjects is evidence of native differences exaggerated by experience. This difference was not only exaggerated, but in some cases reversed. The two who took longest on Problem I profited most from training and were among the first three on Problem IV.

The amount of increase or decrease from problem to problem, as shown in Table 2 and Figure 6, did not correlate significantly with the Kuhlmann-Binet mental ages of our subjects. We might say, therefore, that *within the limits of this group,* especially those ranging in mental age from 2 to 3 years, mental age, as such, did not show a definite relation either to learning on the first problem or to improvement from problem to problem.

It should be noted in this and other references to the influence of mental age that the subjects fall within a narrow range in this respect. It is probable that with a wider spread of mental ages the influence of mental age would be of first importance. Moreover, the probable error of a given mental age, together with the examinational error with low-grade subjects, means that these mental ages are not very reliable for correlation purposes. This low correlation is, therefore, in part due to the unreliability of the measures of mental age as well as to the restricted range of mental ages. What is important is that at this level of intelligence other influences than those measured by mental-age scales are operative and reveal important individual differences which are not obvious from the mental ages alone. Indeed, the well-known importance of the mental-age method of estimating behavioral levels tends to obscure important differences such as are brought out in the present study.

In order to examine this relationship more carefully, we compared these results with the mental age scores obtained by means of the four categories of the Gesell Normative Summaries and on the Merrill-Palmer Scale. The results of these comparisons are given in Table 4. The divergence of scores from one scale to the other confirms the conclusions of the previous genetic study (2). The scores on the Merrill-Palmer are higher than those on the Kuhlmann-Binet. The total Gesell scores are approximately equal to the Kuhlmann-Binet scores. The separate categories of the Gesell scale reveal low language rating and high personal-social rating as compared with the Kuhlmann-Binet. Adaptive and motor ability ratings more nearly approximate the Kuhlmann-Binet.

However, the Merrill-Palmer scores and the Gesell total scores show no more direct relation to learning ability than do the Kuhlmann-Binet. Nor do the separate categories of the Gesell reveal much direct relation to learning ability, either on the first or fourth problems.

TABLE 4

TEST SCORES ON GESELL, MERRILL-PALMER, AND KUHLMANN-BINET

No.	Name	Life age in yrs.	Kuhlmann-Binet age in mos.	Merrill-Palmer age in mos.	Total Gesell in mos.	Gesell in mos.			
						Motor	Language	Adaptive	Personal Social
1	Jimmy	11	39	46	39	40	26	39	50
2	Carl	10	38	49	37	40	24	36	46
3	Dan	9	36	59	42	52	22	42	51
4	Dick	10	36	39	39	40	26	40	50
5	Bobby	8	34	40	38	44	26	30	40
6	Walter	9	33	35	25	21	21	36	21
7	Sammy	10	30	46	35	40	20	39	40
8	Tim	12	28	32	26	24	22	24	33
9	George	12	18	20	22	18	12	23	34
10	Teddy	3	17	18	21	18	10	22	33

We might conclude, therefore, that the homogeneity of the group itself, with respect to mental age, was too great to permit the scale to reveal the differences brought out by the conditioning problem. Or, we might otherwise assume that conditioning is influenced as much by other factors as it is by mental capacity as measured by standard mental tests.

Learning and life age. Inasmuch as Mateer's conclusions relative to the conditioning of defective children suggest a dependence on life age rather than mental age, we studied the life ages of our subjects in relation to learning ability on these problems. Our subjects ranged in life age from 3 to 12 years, with a median of 9.5 years. The life ages were unrelated to mental age. The range in life age was sufficiently great to suggest important differences within certain age limits. Therefore, we divided the children into small groups, according to life age (Table 5).

TABLE 5
SUBJECTS GROUPED ACCORDING TO LIFE AGE

Group 1			Group 2			Group 3			Group 4		
No.	Name	LA	No.	Name	LA	No.	Name	LA	No.	Name	LA
1	Jimmy	12	2	Carl	10	4	Bobby	8	10	Teddy	3
8	Tim	12	7	Sammy	10	5	Dick	7			
9	George	12	3	Dan	9						
			6	Walter	9						

Within these age groups no similarity of performance was apparent. The three who failed Problem I (George, Sammy, and Walter) were in the first two groups, but the two who learned it most readily (Carl and Dan) were also in these two groups. The same condition existed with reference to Problem IV. Therefore, we may conclude that, within the relatively wide range in this small group, life age did not influence learning on the problems. In fact, Jimmy and Teddy, who were at the two extremes in life age as well as in mental age, represented the greatest similarity of performance (Figure 6). The fact that there was some relation between mental age and performance, while there was none between life age and performance, suggests that, in problems of this type, the degree of intelligence as measured by tests is of more consequence than maturity as indicated by chronological age.

Time. Time was taken on each trial, from the moment of the subject's appearance at the entrance to the experimental section to the instant when he raised a box. These limits were set in the attempt to measure only that time during which the subject's attention was centered upon making the choice. This, of course, necessitated the elimination of those activities occurring prior to the actual sight of the boxes or subsequent to the choice.

There were, however, some irrelevant activities occurring within the time measured, such as when the subject attempted to tie a loose shoe-string after starting toward the boxes, tried the door to the observation room, picked up a scrap of string on the floor between the entrance and the table, or found a cookie crumb on the table before raising a box. Such distractions were eliminated as far as possible and extraneous details recorded whenever they occurred unavoidably. These distractions occurred, however, in only four of the trials of the seven subjects who solved Problem I.

Our purpose in timing the trials was to determine (*a*) any individual differences, (*b*) the relation of the first half of the learning period to the second half, (*c*) the relation between rate of learning and average time per trial, and (*d*) the relation between the time taken on successful and unsuccessful trials.

The data considered in this discussion are taken from the results on Problem I of the seven subjects who completed this problem without modification.

TABLE 6
Average Time and Range of Each Subject's Trials through Problem I. Also Average Time of Each Subject at Various Stages of the Learning Process

		Total learning period				
No.	Name	Av. time per trial in seconds	Range in seconds	Last 20 trials in seconds	1st half of learning process in seconds	2nd half of learning process in seconds
1	Jimmy	3.7	2 - 10	3.0	4.2	3.1
2	Carl	4.0	2 - 7	3.5	4.4	3.5
3	Dan	4.3	3 - 10	4.0	4.7	3.9
4	Dick	7.5	3 - 16	7.4	7.2	7.9
5	Bobby	4.3	2 - 10	4.5	4.4	4.1
8	Tim	3.9	2 - 11	3.7	4.2	3.8
10	Teddy	6.0	2 - 21	4.7	7.1	4.9

The average time per trial (Table 6) for individual subjects on Problem I varied from 3.7 seconds to 7.5 seconds. Dick, who took 7.5 seconds, was characteristically deliberate in all his actions. He entered the room, approached the table very deliberately, and looked at both boxes. He raised one slowly, then quickly looked under it, as if playing a game of "hide and seek" with the cookie. Teddy, who took an average of 6 seconds, frequently looked back toward the door as he approached the box, thus retarding his progress. Sometimes he stood at the table for a few seconds prior to making a choice, apparently ignoring the boxes. Such behavior differed from the irrelevant activity previously discussed, in that there was no obvious distraction but probably insufficient incentive to provoke speed. Teddy's playful responses in other problems confirmed this suggestion. The other subjects, who ranged from 3.7 to 4.3 seconds in their average time per trial, showed more variation from trial to trial than from individual to individual.

The range of each subject covered at least 8 seconds, as may be seen in Column 2, Table 6. This is considerably more than the differences between the subjects.

Every child except Bobby decreased his average time on the last 20 trials. That is, the average time required after the response had been learned was less than the average time required through the whole learning period. A comparison of the average time taken during the first and second halves of the learning period revealed that there was a tendency to take less time in the latter half. This obtained for every child except Dick. Inasmuch as the amount of successful trials and also familiarity with the situation increased with the continuance of the problem, it was interesting to speculate whether a shorter time per trial was characteristic of successful trials or of familiarity with the situations.

TABLE 7
AVERAGE TIME ON SUCCESSFUL AS COMPARED WITH UNSUCCESSFUL TRIALS

No.	Name	Successful trials in seconds	Unsuccessful trials in seconds	Difference in seconds
1	Jimmy	3.5	4.2	— .7
2	Carl	3.6	5.1	—1.5
3	Dan	4.2	4.6	— .4
4	Dick	6.8	9.4	—1.6
5	Bobby	4.1	4.8	— .7
8	Tim	3.8	4.6	— .8
10	Teddy	5.9	7.4	—1.5

Subjective observation seemed to reveal that failure was accompanied by more hesitancy and indecision than was success. The only objective evidence of this was in the time taken on successful as compared with unsuccessful trials. This was computed and, as is evident from Table 7, the average successful trial required from .7 seconds to 2.6 seconds less time than did the average unsuccessful trial. This seems to indicate that there was an objective difference in time between failure and success, confirming the observer's opinion that delay more often accompanied failure than it did success.

These results were not sufficiently extensive to treat statistically with any degree of validity. However, possible correlations were considered by inspection and by the method of rank differences. Such methods revealed no tendencies toward correlation between time taken and learning ability, mental age, or life age. Such a correlation might be revealed by a larger group, but the present observations, which, in this respect, are almost purely subjective, indicated that time is more closely associated with personality reactions, temperamental differences, and the incentive value of the lure than it is with learning ability in these problems or with intelligence as measured by mental tests.

From these data we may conclude, with respect to time: (*a*) that there are individual differences in the time taken which can often be traced to characteristic personality reactions, such as slowness or playfulness; (*b*) that the average time per trial decreases in the latter half of the learning period; (*c*) that this decrease seems to accompany the successful rather than the unsuccessful trials; and (*d*) that the average time per trial does not appear to be correlated with the learning ability on the problems, the mental age, or the life age of the subjects.

III

Discrimination Experiments

The Problems. In the second phase of the study we were concerned with (*a*) the use of this method in measuring the discriminatory ability of our subjects, (*b*) the individual differences revealed by the group and their relation to learning, and (*c*) whether discrimination was made on the basis of absolute or relative choice.

The attempt to measure size discrimination was not made until after all the conditioning problems had been completed. Four cubic boxes, 9, 10, 11.5, and 12 cm., respectively, were used, which allowed six combinations, decreasing the difference by .5 cm. for the larger box. Table 8 gives these combinations, their actual difference, and the relative difference with respect to the size of the larger box.

TABLE 8
PAIRS OF BOXES USED IN DETERMINING DISCRIMINATORY ABILITY

Pair No.	Size of boxes in cm.	Difference in linear measurement Absolute	Relative	Distance between boxes
1	9 - 12	3.0 cm.	1/4	25 in.
2	9 - 11.5	2.5 cm.	1/5	25 in.
3	10 - 12	2.0 cm.	1/6	25 in.
4	10 - 11.5	1.5 cm.	1/8	25 in.
5	9 - 10	1.0 cm.	1/10	25 in.
6*a*	11.5 - 12	.5 cm.	1/24	25 in.
6*b*	11.5 - 12	.5 cm.	1/24	55 in.

Five successful trials on one day, made in less than 20 total trials, was considered the criterion of satisfactory discrimination. The box under which the lure was placed was determined by the last problem completed by the child. For example, if the subject completed only the first problem, the discrimination was based on the choice of the smaller box of each pair, whereas, if he had completed the fourth problem, discrimination was based upon the choice of the larger box of each pair.

Whether discrimination was made on the basis of relative or absolute choice was determined experimentally after the first problem. The subject had been conditioned to the smaller of two cubic boxes 12 and 16 cm., respectively, or in the ratio of 3:4. A 9-cm. box was then presented with the 12-cm. box. The ratio thus remained the same, but the smaller box of the former pair was now the larger of the new pair. The subject was given five trials with a reward under each box. The first trial was, of course, most significant. However, four more were given in order to avoid conclusions based on chance success. If the child continued to choose the 12-cm. box, the choice was considered to have been made on the absolute basis. If, on the other hand, he chose the 9-cm. box, the choice was considered relative, since he had chosen not the same box but the one of the same relative size.

Discussion

Relative and absolute choice. After the first conditioning problem, we presented the test for relative or absolute choice, and found that every subject except Jimmy made the relative choice invariably (Table 9). This one exception made his first two choices relatively and then continued to choose the box in the left position, where the smaller box had been placed during the first two trials.

TABLE 9
Results of Test for Relative and Absolute Choice, Showing the Number of Each Type of Choice Made by Each Subject

No.	Name	Relative choice	Absolute choice
1	Jimmy	3	2
2	Carl	5	0
3	Dan	5	0
4	Dick	5	0
6	Walter	5	0
8	Tim	5	0
10	Teddy	5	0
Total		33	2

Such results, however, do not imply that the subjects were unaware of the change. In fact, Carl, as soon as admitted to the room, said "Ooh, a littler one!" Several others looked from one box to the other before choosing the smaller one. Bobby, Tim, and others, however, displayed no overt signs of awareness of a change in the situation. Those subjects who appeared aware of the change did not seem to be influenced by it, inasmuch as all but one made the relative choice.

These results are in agreement with the majority of similar experiments with animals, and the same method might well be applied in determining the basis of other sensory adaptations, such as brightness, color, tones, temporal sequence, and number concept among idiots.

Discrimination. The results on relative and absolute choice indicated the feasibility of decreasing the difference in the size of the boxes, expecting the child to choose the one of the same relative size as that to which he had been conditioned, as long as the difference fell within his discriminatory limen.

Beginning with the 9- and 12-cm. boxes, the difference between the boxes was reduced by steps of .5 cm. for the larger box. The pairs were arranged both in order of absolute and relative difference, although the steps in the latter were not even. It will be noted (Table 8) that the larger box of one pair was often the smaller of the following pair, and vice versa. This never resulted in confusion; if the child could discriminate, he always chose the smaller or larger box, depending on the immediately preceding conditioning.

According to Weber's law, the least perceptible difference in volume, as in other sensory fields, is dependent upon the ratio of the two objects as much as upon their absolute differences. In other words, it should be more difficult to discriminate between 12- and 11.5-cm. boxes than between 10.5- and 10-cm. boxes, although the actual difference in both pairs is .5 cm. Therefore, the relative difference between the boxes is given, although no experimental use was made of the fact nor was such demonstration made as would be necessary in an actual determination of sensory equipment. *It must be remembered that the results of this experiment are illustrative of a method, rather than for the determination of facts.* The discriminatory tests were completed by only seven subjects. It was necessary to discard the results of the remaining three subjects, because of illness. Five of the seven subjects had completed four conditioning problems which left the larger box the most recently conditioned choice. In these cases discriminatory ability was measured by ability to select the larger box of each pair. Two subjects had completed only the first problem, and, therefore, discriminatory ability was based on the selection of the smaller box in each pair. Five correct responses in one day within four days was considered the criterion of discrimination.

After the subjects had discriminated between boxes differing by .5 cm. (Pair 6*a*), these same boxes were placed in Positions 1 and 5, or 55 inches from each other rather than 25 inches, as in the previous presentations. This increase in distance was accompanied by failure in two of the subjects, revealing the fact that the ability to discriminate depended not only on the relative and absolute differences between the boxes, but upon the distance between them. Therefore, we cannot give the specific ability to discriminate differences in size, unless we take into account the distance between the boxes.

The results of these discrimination tests are presented in Table 10. The results indicate that under the conditions of this experiment individual subjects varied from the ability to discriminate a difference of 1/6 in linear dimension to the ability to discriminate a difference of 1/24. The three subjects with the lowest mental ages failed first. The remaining four subjects all were able to discriminate a

TABLE 10

Results of Discrimination Tests Showing Number of Trials Taken in Each Pair of Boxes and the Final Results

		Pair 1		Pair 2		Pair 3		Pair 4		Pair 5		Pair 6*a*		Pair 6*b*	
No.	Name	No. of trials	Result	No. of trials	Result	No. of trials	Result	No. of trials	Result	No. of trials	Result	No. of trials	Result	No. of trials	Result
1	Jimmy	5	+	5	+	5	+	5	+	10	+	15	+	20	—
2	Carl	5	+	5	+	5	+	15	+	5	+	5	+	20	—
3	Dan	5	+	5	+	5	+	5	+	10	+	10	+	5	+
4	Dick	5	+	5	+	5	+	10	+	5	+	10	+	15	+
6	Walter	5	+	10	+	5	+	5	+	5	+	20	—		
8	Tim	5	+	5	+	10	+	5	+	5	+	20	—		
9	George	10	+	10	+	10	+	20	—						

difference of 1/24 at 25 inches, and two were able to discriminate the same difference at 55 inches. Therefore, we may say that, other things being equal, the ability to discriminate seems mildly correlated with mental age. Among these seven subjects there is, also, some relation between ability to discriminate and learning ability as displayed on the first problem.

Considering the number of trials necessary to attain successful discrimination, we found that the easier discriminations were made immediately. However, as the problems became more difficult, the subjects required 10, 15, or 20 trials before obtaining five correct in a single day. This suggested that, with sufficient practice, the subjects might discriminate still further.

This phase of the study, although it did not accurately disclose the discriminatory abilities of the subjects, revealed the feasibility of using this method as a means of determining such ability and of measuring sensory equipment in other fields. It also suggested the possibility of studying the effects of position and practice as influential factors in discriminatory ability.

Summary and Conclusions

Recent literature has revealed an increasing interest in conditioning as a training process, especially among preschool children. The present study was undertaken to establish (*a*) a method of studying conditioning in idiot children who, according to mental tests, are the equivalent of preschool children; and (*b*) a means of estimating the sensory equipment of these children below the mental ages where adequate verbal instructions may be given. Previous studies suggested that the best results could be obtained by eliminating the presence of the observer and using a food lure. These suggestions were adopted in the present study and an attempt made to keep the technique of such a nature that it could be used with both preschool children and the higher apes as well as with idiot children.

The problems consisted of conditioning the subjects to one of two boxes, differing only in size. The boxes were placed on a table, the respective positions varying according to a chance series, and a cookie was placed under one box, to serve as a lure. The subjects were given five trials a day, with ten minutes between each trial. Observations were made through a one-way vision screen, the observer being at no time in the room with the child.

After the first conditioning, the lure was placed under the opposite box and a second conditioning took place which called for the inhibiting of the learned response and the substitution of the opposite one. This was followed by a third and a fourth conditioning, alternating the placement of the lure with each conditioning problem.

These problems were followed by experiments on size discrimination, the first of which was a test to determine whether the selection was made on the basis of relative or absolute choice. This was accomplished by maintaining the same ratio between the two boxes, but using the smaller box of the original pair as the larger box of the new pair.

In the actual discrimination problems, the difference in the size of the boxes was decreased from 3 to .5 cm. by steps of .5 cm. for the larger box. These pairs were presented consecutively and five correct choices on one day made within four days was considered the criterion of discrimination.

The study was by no means sufficiently extensive, nor were there sufficient subjects to permit us to arrive at general conclusions. However, there were specific tendencies which suggested important leads for further research and indicated the importance of the study as a method. These tendencies, stated as a summary of results, were as follows:

1. Our subjects displayed varied abilities in these problems. Three failed entirely until the incentive and procedure were changed, and the remaining seven ranged from 30 to 85 in the number of trials required for completion of Problem I. This range of individual differences increased with successive problems, being greatest on Problem IV.

2. Perseveration, as measured by the number of trials during which a subject continued to make the previously conditioned choice without reward, was prevalent in varying degrees among all subjects. This tendency decreased with additional problems, being greatest on Problem II and almost negligible on Problem IV.

3. Two subjects displayed no evidence of breaking down this perseverative tendency until an exposed lure was presented on the opposite box.

4. The perseverative tendency and its influence on consecutive problems seemed indicative of certain training possibilities, such as adaptability and persistence.

5. In all but two of the subjects choices were temporarily made on the basis of position. These position preferences were strongest

among the three subjects who failed the first problem as originally presented.

6. Position habits were, in some cases, broken down after several consecutive failures resulted, i.e., when, in the chance series, the correct box fell on the side opposite to the position habit for all the trials of one day.

7. The three subjects who failed the problem had the strongest position preferences, two for the left and one for the right position. These three position habits were not broken by consecutive failure or absence, but did break with the change in incentive.

8. Position preferences occurred only in Problem I.

9. There were obvious effects from the change in incentive, but the subjects were too few and the conditions too varied to warrant drawing general conclusions. One subject learned after the quality of the lure was changed and two learned after the lure was placed in plain sight so as to establish more quickly the association between the cookie and the box. Placing the lure on the box was used effectively as a means of breaking perseverative tendencies as well as position habits.

10. Learning ability showed very little relation to mental age within the limits of this group, especially among those ranging between two and three years on the Kuhlmann-Binet scale. However, the three who failed the problem were among the five lowest in mental age.

11. Consideration of supplementary test scores, the Gesell and Merrill-Palmer, revealed the same lack of marked relation to learning ability as was found with the Kuhlmann-Binet. There was, however, a tendency for those who failed to be in the lower half of the group.

12. The life age of these subjects covered a wide range, but there was apparently no relation between life age and learning ability. In spite of the fact that the life-age range was greater than the mental-age range, the relation to learning ability seemed less.

13. Individual differences as measured by speed could often be traced to characteristic personality reactions. However, the range in time taken by any one subject was as great as the differences between the averages of the various subjects.

14. The average time per trial decreased in the latter half of the learning period. This decrease seemed to accompany the successful rather than the unsuccessful trials.

15. The average time per trial taken by individual subjects did

not seem to be related to mental age, life age, or learning ability in these problems, but rather seemed to be determined by personality differences among the subjects.

16. Before beginning the discrimination experiments, a test for relative and absolute choice was made. Changing the actual boxes used, but maintaining the same ratio, showed that eight of the nine subjects chose the box relatively rather than absolutely.

17. The discriminatory ability of these subjects under the conditions of the experiment varied from the ability to discriminate a difference of 1/6 in linear measurement of the boxes to the ability to discriminate a difference of 1/24.

Summarizing these specific conclusions, there are certain general facts which make the study worthwhile. First of all, we have demonstrated a method whereby subjects of this level of intelligence will spontaneously adapt to an experimental situation. This process of adaptation is sufficiently slow to permit analysis and study, which makes it of importance in itself as a study of learning.

Secondly, the end result of this adaptation may be set up so as to reveal the sensory equipment of the subjects by determining, after conditioning, the limen of their discriminatory ability.

The third contribution of this study is the suggestion that idiots, as human subjects of arrested developmental status, may be compared in learning ability and sensory equipment with normal infants and with anthropoid apes. This method, using food as the previously conditioned stimulus and any form of sensory material as the newly conditioned stimulus, is adaptable to the three groups known to be, quantitatively but not qualitatively, at approximately the same level of general behavior.

Numerous suggestions for further research are evident from the present study. With respect to learning experiments, we might present problems of greater difficulty and more complexity and observe those factors which facilitate or retard learning. This is easily accomplished by using more than two boxes and having more than one variable.

A second interesting study might be centered around the incentive. The effectiveness of various incentives could be compared by using two or more and observing the one favored by conditioning.

The measurement of the sensory discrimination of the subjects in various visual fields such as form, color, and brightness is a third possible trend which future research may take. Like the present study, it is expected that each of these suggestions might lead to still other problems.

References

1. Aldrich, C. G. Incentive as a factor in problem solving. *Tr. School Bull.*, 1930, **28**, 121-126.
2. Aldrich, C. G., & Doll, E. A. Comparative intelligence of idiots and preschool children. *J. Genet. Psychol.*, 1931, **39**, 227-257.
3. ———. Problem solving among idiots. *J. Comp. Psychol.*, 1931, **12**, 137-170.
4. ———. Problem solving among idiots: the use of implements. *J. Soc. Psychol.*, 1931, **2**, 306-336.
5. Buhl, G. H. Education of low-grade feeble-minded through job analysis. *Tr. School Bull.*, 1928, **25**, 1-9.
6. Burnham, W. H. The normal mind. New York: Appleton, 1924. Pp. 702.
7. Cowan, E. A., & Carlson, A. D. Bringing up your child. New York: Duffield, 1930. Pp. 378.
8. Cushing, H. M. A perseverative tendency in preschool children. *Arch. Psychol.*, 1929, No. 108. Pp. 55.
9. Doll, E. A. Capabilities of the low-grade feeble-minded. *Tr. School Bull.*, 1923, **20**, 1-12.
10. Goddard, H. H. Feeble-mindedness: its causes and consequences. New York: Macmillan, 1926. Pp. 599.
11. Jasper, H. H. Is perseveration a functional unit participating in all behavior processes? *J. Soc. Psychol.*, 1931, **2**, 28-51.
12. Köhler, W. The mentality of apes. London: Harcourt, Brace, 1927. Pp. 336.
13. Krasnogorski, N. Ueber die Bedingungsreflexe im Kindesalter. *Jahrb. f. Kinderhk.*, 1909, **19**, 1-28.
14. Mateer, F. Child behavior. Boston: Gorham, 1917. Pp. 239.
15. Pavlov, I. P. Lectures on conditioned reflexes. New York: International Publishers, 1928. Pp. 414.
16. Tredgold, A. F. Mental deficiency. (5th ed.) New York: Wood, 1929. Pp. 535.
17. Veith, G. J. Manual training of low-grade defectives. *Psychiat. Quar.*, 1929, **3**, 409-412.

The Training School
Vineland, New Jersey

LE CONDITIONNEMENT SIMPLE COMME MÉTHODE POUR DÉTERMINER LA DISCRIMINATION SENSORIELLE CHEZ LES IDIOTS

(Résumé)

Les débiles mentaux très arriérés fournissent des occasions pour la recherche lesquelles n'ont pas été exploitees antérieurement à cause des difficultés de la technique expérimentale. Cette étude-ci emploie la méthode du simple choix conditionnel comme technique préliminaire pour l'étude de la discrimination sensorielle chez les idiots. On développe cette technique sous le rapport des études comparatives chez les enfants normaux et les anthropoïdes. L'étude fait partie d'une série où il s'agit des aspects psychologiques génétiques, éducationnels et comparatifs de la débilité mentale. On a observé dix garçons idiots des âges mentaux d'un à trois ans et des âges chronologiques de trois à douze ans, dans le développement spontané du choix conditionnel dans la discrimination des cubes de différents volumes. On a employé quatre problèmes dans l'étude des facteurs qui influent sur le conditionnement, le re-conditionnement, et la discrimination. Les résultats rendent plus claires de telles considérations que le stimulant, la persévération, les préférences de position, l'âge mental, l'âge chronologique, le choix absolu et le choix relatif. Les résultats ont aussi quelque valeur pour l'étude de l'apprentissage chez les idiots.

Doll et Aldrich

DIE EINFACHE KONDITIONIERUNG (BEDINGUNG VON REACTIONEN) (CONDITIONING) ALS METHODE ZUR BESTIMMUNG DER FÄHIGKEIT ZUR UNTERSCHEIDUNG VON SINNESEMPFINDUNGEN BEI IDIOTEN

(Referat)

Schwachsinnige auf niedriger Stufe bieten Gelegenheiten zur Forschung die bis jetzt noch nicht verwertet worden sind, weil das Versuchsverfahren hier besondere Schwierigkeiten bereitet. In der gegenwärtigen Untersuchung wird Gebrauch gemacht von der Methode der einfachen bedingten Wahl als vorbereitendes Verfahren zur Untersuchung der sensorischen Unterscheidungsfähigkeit bei Idioten. Dieses Verfahren wird in Bezug auf vergleichende Untersuchungen sowohl an normalen Kleinkinderen wie an Anthropoiden entwickelt. Die Untersuchung gehöhrt einer Serie an die sich mit den genetischen, pädagogischen, und vergleichungspsychologischen Seiten der Geistesschwäche beschäftigt. Es wurden 10 idiotische Knaben geistig 1 bis 3 Jahre und chronologisch 3 bis 12 Jahre alt beobachtet in Bezug auf die spontane Entwicklung der bedingten Wahl (conditioned choice) bei der Unterscheidung von Würfeln von verschiedener Grösse. Es wurden vier Aufgaben gebraucht bei der Untersuchung von Einwirkungen auf die Bedingung (conditioning), auf die Wiederbedingung (reconditioning), und auf die Unterscheidungsfähigkeit. Die Befunde erhellen verschiedene Begriffe wie z.B. die des Antriebs, der Beharrung (perseveration), der Vorliebe für gewisse Lagen, des geistigen Alters, des Lebensalters, und der absoluten vs. der relativen Wahl. Die Befunde haben auch etwas Bedeutung in Bezug auf die Untersuchung des Lernens bei Idioten.

Doll und Aldrich

THE POSTURAL MODEL OF THE HEAD AND FACE IN VARIOUS POSITIONS (EXPERIMENTS ON NORMALS)*

From the Research Department of Bellevue Psychiatric Hospital

Nathaniel Ross

The knowledge we have of our own body is very limited. Experiences are formed in accordance with the circumstances of the actual situation. According to Hartmann and Schilder (5), we do not actually feel the surface of the skin. Cutaneous sensations are localized just below the surface. Skramlik (8-10) has pointed out the fact that we actually feel very little of the face. There are sensations in the regions of the cheekbones, nostrils, and mouth. The fact that the head seems to become narrower when we press it together with both hands is related to the vagueness of our sensory appreciation of the head. Federn (2) reports that when he is tired and grasps his head with his hand he feels as though the part between them had no extension. Goldstein and Gelb (3) have shown that the picture we have of our own body is especially based on optic impressions. When we cannot get clear-cut optic pictures, the knowledge of the body remains incomplete. Kinaesthetic impressions aid in clarifying such knowledge. The analysis of the Japanese illusion by Klein and Schilder (7) has shown that we orient ourselves concerning the body either by making movements or by obtaining actual contact with the surroundings. (The Japanese illusion is brought about by a double crossing of the hands and fingers; the individual is then often unable to make a movement with one of the fingers pointed at, but rarely fails to do so when the finger is touched.)

It is a problem of great interest to determine the principles governing our knowledge of the face and skull. We know how often hypochondriacal sensations are localized in the head. Our consuming interest in looking into the mirror may certainly in part be attributable to our incomplete knowledge concerning the face.

Another problem of importance is concerned with the change

---

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, November 25, 1931.

in the perceptions of the face effected by changes in the relation between the head and body. Investigations by Skramlik and by Hoff and Schilder have shown that we experience our body in various attitudes and postures as though it were in the average attitude and posture. The Aristotelian deception (a ball touching crossed fingers is felt as two balls) shows clearly that we do not acknowledge postures which are far different from the average. Skramlik has examined this phenomenon very carefully.

Hoff and Schilder have shown that motor impulses change the postural model of the body. As they formulate it, the actual postures of the body are transposed in the direction opposite to the pull of the tone.

> "1. There is a body-scheme (postural model) for normal positions; these positions are so firmly established on a sensory basis that deviations are not appreciated to their full extent, whether these are brought about by a movement or by a change in posture. This formulation is based upon the findings and interpretations of Skramlik. 2. The tonic phenomena of induced tonus in Goldstein's sense, of the tonus of the static and postural reflexes on the basis of persistence of tone, and the "convenience" reactions, change the postural model in the direction of their pull. The true positions of the body are consequently experienced as changed in a direction opposite to the pull of the tonus. The extremities are thus experienced as changed from the normal position, when there exists a motor tendency to the restitution of the normal position. 3. The principles described under 1 and 2 work in opposition to each other in relation to the perception of the positions and postures of our own body" (6, p. 61).

There are additional principles to consider. The so-called postural model of the body, it has been emphasized, is in many respects based upon our optic perceptions and images. According to Claperède, we localize our ego in the middle of the forehead and make observations from this point. It is not difficult, from this point of view, to understand why, in Gemelli's experiments on two-point discrimination, the perception of the same distances should vary with the position of the outstretched arm. The variations can be understood as effects of perspective with the point of observation approximately at the middle of the forehead. We also observe ourselves, however, from a point outside our body, especially when we wish to visualize our actual appearance.

In investigating these problems in connection with the postural model of the head and face, the following procedure was adopted:

There were four subjects, two men and two women. Of these, three are physicians (including the writer) trained in experimental psychology, and one is a lay individual without any previous experience of this nature. The subjects are designated as A, B, C, and D.

The so-called normal position of the head was designated as the one in which the subject faced forward while sitting straight up. The head was held so that a plane passing tangential to the eyes was parallel to the coronal plane of the body. With eyes closed the subject was asked to describe:

1. What he actually felt on various parts of the head, including the vertex, occiput, eyes, nose, chin, mouth, forehead, malar regions, and other parts.
2. The accompanying visual impression.
3. The location of the sensation with reference to the surface.
4. The constancy or variability of the impression.
5. The size, contour, and difference in quality and intensity of sensation of the respective sides of the head and face and their different parts.
6. What is experienced between the parts actually felt.
7. The relation of the head to its immediate surroundings.
8. The relationship of the coronal plane of the face to that of the body. This plane of the face was considered as one passed tangential to the cheekbone regions.
9. The antero-posterior diameter of the head, an approximate line passing from the bridge of the nose to the occiput.

(In the first experiment the subject was asked to try to form an impression of this diameter and in subsequent ones to give the relative lengths.)

10. The lines between the eyes, cheekbones, and corners of the mouth. These were shown by means of a pencil.
11. The relative distance from the known midline of the eyes, cheekbones, and corners of the mouth.
12. The so-called "vertical" relations of the features—the relative distances between them from the highest point of the forehead to the chin.
13. The direction in which the eyes seem to be looking. This was compared with the actual position of the eyes obtained by

quickly lifting the subjects' eyelids. Two of the subjects, A and B, have a muscle imbalance which manifests itself by an outward deviation of the left eye upon an attempt at strong convergence. In D there is a lack of converging power. The subjects were asked to show with linear objects such as pencils, the line of vision of each eye.

Observations were made in the following positions:

1. The normal.
2. Head turned as far as possible to the right.
3. Head turned as far as possible to the left.
4. Head bent as far as possible to the right without rotation.
5. Head bent as far as possible to the left without rotation.
6. Head flexed forward as far as possible.
7. Head extended backward as far as possible.
8. Trunk flexed as far as possible with head hanging down.
9. Lying prone on a flat surface with head in "normal" alignment with body.
10. Lying down, head turned as far as possible to the right.
11. Lying down, head turned as far as possible to the left.
12. Lying down, head flexed forward as much as possible.

The protocol of Experiment III is given in full, summaries being presented of the remaining experiments.

EXPERIMENT I—HEAD IN "NORMAL POSITION"—SUMMARY

On the whole we find that the face is experienced over bony prominences such as the cheekbones, forehead, etc., and areas of contact, such as the eyes and mouth, as areas of warmth, tingling, tension, or contact which are felt below the surface and are incessantly changing in their size, contours, and intensity of perception. At the base of the skull or deep within the head, there is a feeling of weight and pressure. Between the regions actually felt there is experienced a smoky, gaseous stuff which in three cases out of four is not distinguished from the external surroundings, and in the fourth case is only poorly so demarcated. In three cases the face is felt to be asymmetrical, and the vertical relations altered in the region of the eyes and malar bones.

The knowledge we have of our face, then, is a most incomplete and shifting one. Our findings in this experiment agree with those of Hartmann and Schilder and of Skramlik.

It is interesting to note that, although all subjects give the lines of vision (usually convergent) as starting from within the head, they describe various regions of the face as though these were being observed from without. In no case in the following experiments, as well as in this one, do the subjects indicate that they are conscious of these two points of observation. This is an important phenomenon which will be especially discussed.

Experiment II—Head Turned to the Right—Summary

In summary, we find in this experiment that when the head is turned to the right, the right side of the face is experienced as narrower in three cases. It is significant, however, in the fourth case, C, that the right eye and cheekbone are felt to be nearer to the midline, and that the right side of the forehead, mouth, nose, and chin are compressed towards the midline. The right side of the face bulges forward in A and C and the left side in B and D. The plane of the face in A and C is parallel to the coronal plane of the body; this is consistent with the difference in convexity described. In B the plane of the face is almost parallel to the coronal plane of the body, and this is also consistent with the finding in this case that, while the left side of the face has increased in convexity, the right side has also done so. D experiences the plane of each side of the face as being at a right angle to the other, and, as a matter of fact, the result is at variance with that in the other cases, for the whole head is then experienced as being twisted over farther to the right than it actually is.

The lines of vision diverge in A, C, and D. In A the left eye looks straight forward and the right almost at right angles to it, towards the right. This is reversed in C, in whom the right eye looks straight forward, and the left sharply to the left, parallel to the coronal plane of the body. In D the left eye looks straight forward, and the right towards the right at 60° to the sagittal plane of the body. In B both eyes look straight forward.

Actually the eyes in all subjects are deviated to the right completely or almost so.

Except in the case of C, in which the possible discrepancy in the experiences concerning each side of the face as a whole, and those of the individual features, has been pointed out, the contraction and difference in convexity of the respective features are consistent with those of the corresponding side of the face. An apparently unimportant exception is to be found in B, who finds the right side generally contracted, but the right side of the mouth longer. Further, C finds the plane of the face to be parallel to the coronal plane of the body, but the eyes, cheekbones, and mouth are in a plane which is at a greater angle to the body plane. Such apparent inconsistencies are by no means rare. Evidently the forces which activate these subjective phenomena are effective between wide limits, vary with the time of observation, and produce varying results.

The optic impressions are those of vague, gray, misty masses in which the features appear as lines, areas, or prominences poorly distinguished by their varying darkness or lightness. The asymmetries accord with those of the tactile impressions. Generally the features are visualized as larger than they are felt. The side of the face which is felt to be smaller is usually seen as darker.[1]

Experiment III—Head Turned to the Left

It is remarkable that when the head is turned to the left, there is a unanimity of experience concerning the type of asymmetry of the face. Every subject now finds that the left side is narrower, more compact, more intensely felt, and more convex anteriorly, as compared with the right side and the left in the "normal" position. Associated with the feeling of largeness and flatness on the right side, A, B, and D mention a feeling of "lightness," and, conversely, on the left, one of darkness. It is B's and C's experiences in this experiment which are not analogous to those in Experiment II. There B had found that, while the right side of the face was narrower, it was less convex than the left. Now the side towards which the head is turned is more convex. C had found that, while the right side of the face in Experiment II was more convex, it was wider. He nevertheless experienced the contraction and approximation to the midline of the features on the right side. In Experiment III, he finds the left side to be not only more convex but narrower.

The findings concerning the plane of the face, the A-P diameter of the head, and the feeling in the region of the vertex are the same as in Experiment II.

In all cases, the left eye is smaller, narrower, or more compact, more bulging, more intensely felt, and closer to the midline than the right.

---

[1] With rare exceptions this statement holds true in the succeeding experiments, and will not be repeated.

The left side of the nostril in A and C is compressed. B feels that the nose is further to the right than it really is—an impression analagous to the one this subject had in Experiment I, where the sagittal plane of the nose is described as being at a smaller angle to that of the body than it actually is. D's experience is confined to the fact that the inspired air is felt more intensely on the left side.

The left side of the mouth in A, C, and D is smaller, more convex, more intensely felt. In B it is also more clearly felt but is longer and less convex. Analogously, B felt the right side of the mouth to be longer and flatter in Experiment II.

The left cheekbone region is in all cases felt as more convex and closer to the midline than the right. The increase in convexity on the left is so marked in C that he has the impression that it is actually anterior to the right, although he found the plane of the face to be parallel to the coronal plane of the body. A and D experience the left cheekbone as smaller, darker, more compact, more intensely felt than the right.

In C the left side of the chin is compressed towards the midline, in D there is a feeling of tingling only on the left side. A and B cannot localize the vague area of tingling in this region.

In all cases the forehead is felt as compressed towards the midline on the left.

The lines joining the eyes, cheekbones, and corners of the mouth vary as before between those in C, which are at a relatively small angle to the actual line, to those in A which are in a plane parallel to the coronal plane of the body. Again the lines are in the same plane, with one exception, in this instance C. Previously B had found that the line joining the corners of the mouth approached the coronal plane of the body more than the other lines. This is now true of C instead.

The vertical relations of the features do not differ from those in Experiment I.

The eyes look straight forward, perpendicular to the coronal plane of the body, with the lines of vision parallel in B. In C they look almost straight forward, the lines of vision diverging a little. In A the right eye looks straight forward and the left in a line divergent from it but not as much as in Experiment II. In D the lines of vision converge sharply, with the left eye looking straight forward and the right looking sharply to the left in a plane parallel to the coronal plane of the body.

Experiment III—Head Turned to the Left—Summary

It is worthy of note that the findings in this experiment are not exactly analagous to those in Experiment II. It is clear that there is a multiplicity of factors operating with different intensities at various times. We find that, on the whole, the side of the face that is turned away from the observer is experienced as laterally compressed, smaller, darker, more intensely felt, and more convex anteriorly, with the features on that side closer to the midline or compressed towards the midline, and individually smaller and more compact than those of the opposite side. Except in D, the A-P diameter of the head is shortened and the plane of the face is parallel or almost parallel to the coronal plane of the body. D speaks of a plane for each side of the face, one perpendicular to the other, one being parallel to the coronal plane of the body. This subject, however, finds that the lines joining the eyes, cheekbones, and corners of the mouth are in a plane intermediate between the actual one of the face, and the coronal plane of the body. On the face of it, this is contradictory to the previous statement that one side of the face is parallel to the coronal plane of the body and the other side perpendicular to it, for in such case the lines between the eyes, etc., would be in a plane at a greater angle to the coronal plane of the body than the actual one. I do not believe, however, that we can, in these experiments, speak of "contradictory" experiences. We must speak of "different" experiences. In this case, we have three different experiences, in a literal sense contradictory, which are nevertheless capable of existing together in one individual. There is the actual knowledge concerning the face, there is the impression that the face is twisted around more sharply than it really is, and there is the impression, that when looked at from another point of view, the face is less sharply twisted about than it really is.

The tendency to feel that one or both eyes are looking straight or almost straight forward is again present. Where both do not actually seem to be looking straight forward, it is the right in this experiment which does so, except in the case of D. Here it is the left eye which does so, while the right feels as though it were looking sharply to the left. Perhaps this is related to D's unique tendency to feel that the head is twisted more sharply than it actually is. Actually, the eyes are deviated to the left.

Experiment IV—Head Bent to the Right—Summary

In this experiment there is a general tendency to regard the head as contracted in all diameters on the right, with convergency on this side towards a blunt apex. In C, who has the opposite experience, the tendency comes out in the relationship of the features to each other, "vertically," and with reference to the midline. There is a feeling of weight on the dependent side of the head. In isolated instances it seems as if a part of the head, such as the occiput, has slipped past to the right of the midline. In an isolated way also, there appears the tendency to experience the head as though it were striving to return to the original position; for instance, A feels that his eyes are looking straight forward in a horizontal plane, and B feels that the lines of the eyes, cheekbones, etc., are parallel to the ground. The struggle between the conflicting tendencies is clearly shown in varying experiences concerning the lines between the features and the practically identical ones concerning the "vertical" relations.

The eyes in A look straight forward in a plane almost parallel to the horizontal. This is not in accord with his experience that the left eye seems to be higher and the right lower than they actually are. In B they look straight forward and in a plane parallel to the ground, again a contradiction to the apparent location of the eyes. In C the right eye looks downward and 40° to the left, and the left looks downward and 40° to the right. In D the right looks downward 5° and 40° to the left. The left looks up and 30° to the right. Actually, the eyes are deviated to the right and upward.

Experiment V—Head Bent to the Left—Summary

The conflict between the various tendencies comes out sharply in this experiment, in which the findings are by no means exactly analogous with those of the previous one. On the whole, the head is experienced as a cone with curved sides converging to a blunt apex on the left. The left side of the head, like the right in Experiment IV, is contracted in A, B, and D, and larger in C. The compression tendency does not come out in C, as it did in the previous experiment. He experiences the left eye and cheekbone now as farther from the midline, the left side of the nares as wider, the features as farther apart. In B the tendency away from the midline is expressed by the increased length of the left side of the mouth. In D, contrasting with the contraction of the left side of the face, the left side of the forehead is longer, the left nares wider, and the left eye and cheekbone are farther from the midline. Again, in contrast with the general convergency towards the left, the features on the left are not closer to each other, and the lines of the features are parallel to each other in this subject. Evidently the tendency to regard the parts of the face as being in their known relationships comes out strongly in this case.

In several instances certain parts of the head seem to have slipped to the left of the midline, such as the occiput in A and C, and the vertex in B. As in Experiment IV, there are isolated cases in which parts of the head "strive" to return to the original position. In A the lines of vision are in the horizontal plane. In C the lines between the features are practically horizontal, and in B the sagittal plane of the nose is in the vertical plane.

In B, C, and D the lines of vision converge to a point in front of the face. In Experiment IV this convergence was expressed in C and D. Actually, the eyes are deviated to the left and upward.

Experiment VI—Head Bent Backward—Summary

In general, there is a feeling that the head has become deepened by the bulging backward of the occiput and the bulging forward of the forehead. Two opposing tendencies are revealed in the feeling of B and D, on the one hand, that the chin is more prominent and pointed, and that of A and C, on the other, that it is flattened and receding.

The approach of the plane of the face to the coronal plane of the body is clearly expressed in all the subjects; in fact, in C there is actually a tilting forward. Following the same tendency, the eyes in B and C look in a plane close to the horizontal. In A they actually look downward. In D the struggle of ambivalent forces appears in the unique experience of this subject that there are two lines of vision for each eye, one directed upward and one downward. Ambivalence is also expressed in A, B, and C, who feel the eyes and cheekbones closer together and the lower features farther apart.

The eyes look straight forward a little above the horizontal plane in B and C. In A they look downward, a little below the horizontal. In these three subjects the lines of vision are parallel. D has the curious feeling that there are two lines of vision for each eye, one directed upward and the other downward. These lines converge, those looking downward more sharply than the others.

Actually, the eyes are directed upward.

EXPERIMENT VII—HEAD BENT FORWARD—SUMMARY

The foreshortening of the face appears in three subjects, and the sense of weight and bulging of the features in all. The feeling that the face is lengthened is peculiar to D. The widening in A, B, and C is probably relative to the foreshortening. A contrasting tendency appears in B, who finds that, while the face as a whole is wider, the eyes and cheekbones are closer together and the mouth narrower. The widening and lengthening of the lower part of the face in D is difficult to explain. It is also difficult to understand why D feels that in this, as in the previous experiment, the face narrows towards the forehead.

Only in B does the plane of the face approach the coronal plane of the body. In A and D it is felt as the actual one, in C as actually titlted downward. In A, B, and C the eyes look straight downward at the ground, or almost so. Actually, the eyes are directed upward. In D the lines of vision are complicated; it may be noted, however, that one eye looks upward. The fact that there is little tendency here expressed to feel as though the head were in the "normal" relationship with the body may be dependent upon the orientation afforded by the contact of the chin with the chest.

EXPERIMENT VIII—TRUNK FLEXED, HEAD DOWN—SUMMARY

In general there is a vivid feeling that the vertical region is a large, heavy, bulbous mass. The face is generally foreshortened, and where it is not, in D, there is a vertical compression of the features, the nose is shorter and retrousse, and the chin more convex towards the mouth. The tendency of the features to be displaced towards the most dependent part is, in addition, expressed by the peculiar feeling that the eyes are slipping towards the vertex. The widening of the face is relative to the shortening. In D, the fact that the eyes and cheekbones are closer to each other may be in some way related to the experience that the face is longer, or possibly the increased height of the forehead may be so great as to increase the total length of the face.

Partial tendencies towards experiencing the plane of the face and the lines of vision as though the head were approaching the normal position are present in B and C. In the former, the plane of the face is felt as more horizontal than it actually is, and in the latter the lines of vision are in a plane intermediate between the actual one and the horizontal, and directed forward.

The predominant tendency is to feel that the eyes are looking straight downward. Actually, they are directed toward the forehead. In A they look at the nose. The feeling that the eyes are looking downward is probably related to the feeling that they are slipping through the head. In fact in C, two tendencies are expressed, coincidentally. If the eyes tend to look forward and the lines of vision are in a plane approaching the horizontal, the eyes must also look through the head. In D, if the eyes look downward, they cannot, literally, look through the head unless the plane of the face coincides with, or crosses, a plane parallel to the coronal plane of the body. The fact that D experiences the eyes as looking straight downward and through the head and feels that the plane of the face coincides with the actual one shows how the different factors producing these subjective impressions can manifest themselves in the same individual in a complex way which we should not regard as contradictory.

Experiment IX—Lying Flat on Back—Summary

There is less diversity among the subjective impressions in this experiment than in the previous ones. The general feeling is that the head is greatly foreshortened, the face is larger in both dimensions and flatter, and the features are flattened, farther apart in both dimensions, and more diffuse. B and D express an additional tendency to feel as though there were a ridge in the midline. (The prominence of the midline plays an important part in certain disturbances found in hysteria and the aphasias.)

Certain of the features in C do not follow the general trend to enlargement. The forehead is lower, and the nose, mouth, and chin are smaller. The features are also in practically normal relationship. It is difficult to say whether this decrease in size is only relative, or whether it represents an attempt to resist the general diffusion of the landmarks of the face.

Experiment X—Lying Flat on Back, Head Turned to Right—Summary

In this experiment, we see chiefly expressed, on the one hand, a tendency to compression of the head from the right towards the midline, and, on the other, a tendency for the various parts to shift to the more dependent side. The left side of the face is generally lighter and larger than the right, but in D it also sinks to the right and is narrow and concave. In C the features on the left, following the tendency of that side of the face to expand outward, are farther from the midline. An additional factor producing asymmetry of the features is the compression from right to left. This is generally overcome by the gravity effect, but in A it predominates. Despite the orientation afforded by the contact of the right side of the head with the surface on which the subject lies, there still is expressed the tendency of the head to function as though it were in the normal relationship to the body. In B and C this comes out in the divergence of the lines of vision. The line on the left thus approaches the "normal." (Actually, the eyes are deviated to the right.) Again in this respect the opposite factor also comes out—in B, for example, the lines between the eyes, etc., are in a plane inclined towards the horizontal, but the plane of the face is experienced as the actual one.

The features on the right side in B are closer together while in D they are farther apart. In the former the factor of perspective, which will be discussed later, is probably responsible for the distortion. In the latter it is suggested that perhaps the principle at work is the longitudinal spread of the face by compression.

Experiment XI—Lying Flat on Back, Head Turned to Left—Summary

The factors evident in this experiment are the same as in the previous one. The individual experiences are, however, by no means analogous, in this analogous position. All subjects agree on the compression of the left side of the face, and, strikingly, on the closer proximity of the features on this side. There is a more marked tendency to feel the shifting of the parts to the more dependent side in the case of the occiput, nose, and chin, but the compression tendency is more evident in the eyes and cheekbones. In B and D there appears the impression that the head is twisted more to the left than it actually is. In previous experiments this phenomenon is present but infrequently. Here the opposite tendency also comes out in these subjects, when they feel that the line between the cheekbones or corners of the mouth is more horizontal than the actual one. Only in C does the tendency to function as though the head were in the normal position come out in the lines of vision.

Experiment XII—Lying on Back, Head Flexed Forward—Summary

The face is, in general, felt to be wider and longer. The plane of the face is experienced as perpendicular to the coronal plane of the body, and the eyes look forward in the horizontal plane or downward. Actually, the eyes are in a position between extreme upward deviation and the mid-position. The forehead and cheekbones and, in one case, the nose, bulge forward. In C the forehead recedes; the vertex, however, is bulging.

This position is one of intense strain and, in fact, represents the first motion in arising from the prone to the sitting position.

Conclusions

The factors involved in producing subjective impressions of the face and head are apparently the following:

1. *Perspective.* It is as if one were observing his own face from an external point. This point varies. When the head is turned or bent to the side, the observation point is evidently directly in front of the subject. Actually, an observer who looks at the subject whose head is turned to the side sees only one side of the face, the side which in our experiments is felt to be larger. But the other side cannot be entirely negated. It is felt to be smaller. This illustrates the tendency to compromise between the extreme effect of one tendency and the actual knowledge of the face. Such compromises are extremely common in our experiments and appear especially in the experiences concerning the plane of the face and the lines of vision. Skramlik has made previous mention of this phenomenon.

With the head bent back, the point of observation appears to be directly above the subject. Actually, from such a point the perspective effect is one of lengthening. Probably the pull of the anterior muscles of the neck on the face is an additive force.

With the subject lying down, the point of observation is evidently directly above the face, with the compromise tendency again playing an important part. The subject, whose face is turned to the side, feels the uppermost side to be larger than the one which is dependent. One subject, D, in Experiment XII, has the opposite experience. The uppermost side of the face is felt to sag in towards the midline when the face is turned to the right. The effect of gravity, which will subsequently be discussed, evidently overcomes the effect of perspective. With the head turned to the left, however, this subject experiences impressions referable to the effect of perspective.

2. *Tendency to feel the head as though it were in the "normal" position.* When the head is turned to the side, or bent forward or back, the prevailing impression is that the plane of the face is either parallel to the coronal plane of the body or intermediate between this plane and the actual one. The lines of vision are generally those which would exist in the fundamental position. When this is not true of both eyes, it is generally true of one eye. This is again an expression of the compromise tendency. Actually the lines of vision would be markedly different from those imagined. It may briefly be mentioned that the lines of vision in D are frequently at variance with those in the other subjects. Usually there is sharp convergence to a point a short distance from the face, and often the lines are not in the same plane. It would not be within the scope of this paper to discuss this variation, except to say that this subject has a muscle imbalance in both vertical and horizontal planes.

The feeling that the head is in direct alignment with the body when it actually is not is one of extreme importance. It is evidently related to the tendency previously discussed to assume an observation point by an onlooker squarely in front of the subject. It is difficult to resist the suggestion that this is a protective mechanism. Certainly it is related to the peculiarities of the postural reflexes, exhaustively studied by Hoff and Schilder. In addition, it seems to be connected with the changes in bony growth of the face in torticollis, in which, as is well known, there is evidently resorption of bone in the side of the face turned away from the observer, and increase in growth on the opposite side. The type of asymmetry observed is in striking accord with that subjectively experienced by our subjects, and is, we feel, related to both factors just discussed.

3. *Gravity.* It has been pointed out by Hartmann and Schilder that the head is experienced as a heavy mass which behaves like a fluid. Thus, when the trunk is flexed, there is a feeling of great

weight in the dependent head, which feels considerably bulged out and enlarged. It is as though the heavy fluid were trying to burst through the containing shell. At the same time the face becomes foreshortened. We feel that this expresses a downward flow of the hypothetical stuff. When one lies flat on his back, this stuff, figuratively speaking, flows to the back of the head, and the antero-posterior diameter of the head is considerably decreased. At the same time the features are flattened and spread over a large area. It is as if one flattened a ball of putty. There is, in addition to the effect of gravity, apparently an optic factor which produces the impression of the widening and flattening.

Various other phenomena may be accounted for in this way, such as the displacement of the occiput towards the side to which the head is bent, or the side to which it is turned when the subject is lying down. The individual features may follow this flow while counteracting tendencies affect the impression of the face as a whole. Thus, when the head is turned to one side, with the subject in the prone position, the effect of perspective and the contact of the side of the head with the supporting surface is to compress the dependent side of the head. However, the lower eye may be displaced away from the midline and the upper eye towards it because of the effect of gravity. Such an effect upon the individual features is very clearly seen in the position of flexion of the trunk, in which the eyes may seem to be slipping down into the head. Apparently the flow may take place not only in moving features, such as the eyes, but in immovable ones such as the cheekbones, although it is most marked in the former.

4. *Muscle strain.* Hartmann and Schilder have observed that when muscles are contracted, they, and parts proximal to them, seem to be heavier and smaller. Such a factor evidently adds to the effect of perspective, which results in the foreshortening of the face when the head is flexed. It enhances the effects of gravity and perspective in the asymmetry observed when the head is bent to the side.

5. *Contact of part of the head with another surface.* This produces the effect of much greater compression than can possibly exist, as Skramlik has observed. When the head is flexed so that the chin touches the chest, compression at the chin adds to the effect of perspective. This is also true of the contraction of the dependent side of the head in Experiments X and XI.

6. *Imaginary completion of an initiated movement.* In Experi-

ment XII, in which the head is sharply flexed upon the prone body, the subjects feel as though the plane of the face were actually vertical and the lines of vision approaching, or coincidental with, those of the head in the upright position. As a matter of fact, the flexion of the head is the first movement in sitting up, and one may interpret the findings in this experiment as the imaginary completion of the action. The position assumed by the subjects is an extremely uncomfortable one, and probably an emotional factor helps to produce the final subjective impression.

7. *The optic impressions shape the tactile effects.* In other words, the subject feels the various parts of the face and head where he observes them to be from the external point of observation. Gemelli's observations, previously cited, lead to the some conclusion.

It is worthy of note that the subjects frequently complained of a feeling of strangeness and discomfort in describing their impressions. Occasionally there would be a protest against the bizarre appearance of the head. It was felt that there were other possible subjective impressions but, when the first impression was once articulated, the others seemed to become much more vague.

The conflict can be expressed, from one point of view, as one between the subjective impression and the actual knowledge of the head. In fact, there is a feeling of double personality. This is related, more pertinently, to the two localization points of the ego, one from within the head, and one from an external point. It is remarkable that the subjects are unaware of this external point of observation, while they describe its effects. This important phenomenon in the normal, we feel, furnishes a clue to the psychology of the states of double personality observed in organic as well as functional disturbances.

Such mild emotional attitudes lead us to a consideration of pathological changes in the postural model of the body, and especially of the head, which occur in hypochondriasis, schizophrenia, depersonalization states, and delirium tremens. The relation between the normal and the pathological is clearly evident here. We are justified in maintaining that the bizarre, distorted, subjective and apparently objective impressions of the human body, and the feelings of strangeness, fear, and distress, and double personality accompanying them, are exaggerations of phenomena which appear in the normal individual.

References

1. Claparède, E. Sur la localization du moi. *Arch. de psychol.,* 1924, **19**, 172-182.
2. Federn, P. Narzissmus im Ichgefüge. *Int. Zsch. f. Psychoanal.,* 1927, **13**, 420-438.
3. Gelb, A., & Goldstein, K. Zur Psychologie des optischen Wahrnehmungs- und Erkennungsvorganges. *Zsch. f. Psychol.,* 1919, **83**, 1.
4. Gemelli, A. L'influence exercée par la position des parties du corps sur l'appréciation des distances tactiles. *Arch. ital. de biol.,* 1914, **61**, 282-296.
5. Hartmann, F., & Schilder, P. Körperinneres und Körperschema. *Zsch. f. d. ges. Neur. u. Psychiat.,* 1927, **109**, 666-675.
6. Hoff, H., & Schilder, P. Die Lagereflexe der Menschen. Klinische Untersuchung, über Haltings und Stellreflexe und verwandte Phänomene. Wien: Springer, 1927. Pp. iv+182.
7. Klein, E., & Schilder, P. The Japanese illusion and the postural model of the body. *J. Nerv. & Ment. Dis.,* 1929, **70**, 241-263.
8. Skramlik, E. v. Varianten zur Aristotelischen Täuschung. *Pflüg. Arch. f. d. ges. Physiol.,* 1923, **201**.
9. ————. Lebensgewohnheiten als Grundlage von Sinnestäuschungen. *Naturwiss.,* 1925, **13**, 117-122, 134-142.
10. ————. Über Tastwahrnehmungen. *Zsch. f. Sinnesphysiol.,* 1925, **56**, 256-280.

Bellevue Psychiatric Hospital
New York City

LE MODÈLE DE POSTURE DE LA TÊTE ET DE LA FIGURE EN DIVERSES POSITIONS (EXPÉRIENCES AVEC SUJETS NORMAUX)

(Résumé)

Le modèle de posture du corps est un terme employé pour signifier l'image de l'individuel de son propre corps. Ce concept a été surtout élaboré par Schilder. Lui et Hartmann, et Skramlik ont montré comme notre connaissance réelle du corps est vague. Elle dépend en grande partie de nos impressions optiques (Goldstein et Gelb). Nos sensations kinesthésiques et tactiles la rendent plus claire (Schilder et collègues).

Il s'agit dans cette enquête du problème spécial du modèle de posture de la tête et de la figure. On a demandé à quatre sujets normaux de décrire leurs impressions de la tête et les relations de ses parties quand ils ont maintenu la tête en diverses positions. Ils ont éprouvé des distorsions surprenantes. Les principes déduits de l'analyse de celles-ci sont les suivantes:

1. La tête est comme si l'on s'observait de divers points extérieurs. La perspective qui résulte est modifiée par des essais de rectifier la distorsion et de voir la tête encore en alignement avec le corps.
2. La tête se comporte comme une masse coulant vers des parties dépendantes.
3. Les parties proches à des muscles contractés semblent plus petites et plus lourdes.
4. Le contact produit l'effet de compression.

5. Quand on initie un mouvement, on voit la tête comme si elle était dans la position complétée.

L'auteur conclut que les distorsions du modèle de posture de la tête, observées si fréquemment dans la hypochondriase, la dépersonalisation, la schizophrénie, etc., ne diffèrent pas essentiellement de celles montrées chez les sujets normaux.

Ross

DAS POSITURABBILD DES KOPFES UND DES GESICHTS BEI VERSCHIEDENEN HALTUNGEN

(Referat)

Das Positurabbild des Körpers ist ein Ausdruck, der den Eindruck, den das Individuum von seinem eigenen Körper hat, bezeichnet. Dieser Begriff ist besonders von Schilder entwickelt worden. Er und Hartmann, und auch Skramlik, haben erwiesen, wie unklar unsere eigentliche Kenntniss des eigenen Körpers ist. Sie stützt sich grossenteils auf optische Eindrücke (Goldstein und Gelb). Kinästhetische und Tastempfindungen erläutern sie (Schilder und seine Mitarbeiter). Diese Untersuchung bezieht sich auf die spezielle Frage des Positurabbildes [d.h. die innere Vorstellung der Positur] des Kopfes und des Gesichts. Man bat 4 normale Vpp. ihre Eindrücke vom Gesicht und von der Beziehung der Kopfteile während der Kopf in verschiedenen Lagen gehalten wurde zu beschreiben. Es wurden überraschende Verzerrungen erprobt. Die Begriffe die sich aus der Analysierung dieser Verzerrungen herleiten lassen sind folgende: (1) Der Kopf wirde empfunden als ob man sich von verschiedenen äusseren Stellungen aus betrachtete. Das resultierende Perspektivenbild wird abgeändert durch Versuche die Verzerrungen zu rektifizieren und sich den Kopf als in einer geraden Linie mit dem Körper vorzustellen. (2) Der Kopf benimmt sich als eine den hängenden Teilen entgegenfliessende Masse. (3) Teile, die neben zusammengezogenen Muskeln liegen kommen uns kleiner und schwerer vor. (4) Die Berührung bewirkt den Eindruck der Pressung. (5) Wenn eine Bewegung angefangen wird, wird der Kopf als ob er in der vollendeten Stellung wäre visualiziert.

Der Verfasser ist der Meinung, dass Verzerrungen der Vorstellung der Positur des Kopfes wie man sie so oft in der Hypochondriase, bei Verlust der Persönlichkeit, in der Schizzophrenie, u.s.w. beobachtet, sich nicht wesentlich von den an normalen Menschen bewiesenen unterscheiden.

Ross

THE EFFECTS OF COACHING SUBJECTS IN THE RECOGNITION OF FACIAL EXPRESSIONS*

A REPETITION OF PROFESSOR ALLPORT'S EXPERIMENT AND A CRITICISM OF HIS CONCLUSIONS[1]

From the Psychological Laboratories of the University of Nebraska

ARTHUR JENNESS

ABSTRACT

A wide range of individual differences in the ability to identify facial expressions of emotion has been reported. As evidence that these inequalities are due in large part to differences of practice in reacting to expressive criteria, rather than to innate factors, F. H. Allport has offered the data from an experimental observation of the effects of training subjects of varying ability for fifteen minutes in the analysis of facial expressions. The gain in score made by his subjects on the facial expression test after the training period was roughly inversely proportional to their original standing; i.e., the less proficient persons gained the most and the more proficient gained the least. Repeating this procedure with larger groups of subjects, both with the same and longer study periods, the writer largely confirmed Allport's data. However, a control experiment, affording no opportunity for study, yielded similar data, except that on the average no gain in score resulted. The inverse relationship between original ability and amount of gain or loss should not, therefore, be attributed to the effects of training. Further analysis shows that when the reliability of the facial expression test, found by retesting a group of subjects, is taken into account, the negative coefficient practically disappears. The negative coefficient, upon which Allport bases his theory, is thus found to result in this study from the erroneous assumption that the reliability of the facial expression test is perfect.

---

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, August 26, 1931.

[1] Portions of this article are taken from Part II, Chapter 3, of *Experimental Studies of Response to Social Stimulation,* a Ph.D. dissertation on file at the Syracuse University Library. Other data and statistical treatment have been added independently by the writer since the filing of the dissertation. The writer is under obligation to Professor F. H. Allport, who supervised the dissertation, and to Professor Ernst Thelin, for valuable aid and criticism.

The writer, though inclined to agree with Allport's theory, concludes that this type of experimental evidence, when closely scrutinized, fails to support the theory.

"How do individuals differ in their ability to name facial expressions: and to what are these differences due?"

Under this heading, in *Social Psychology* (1, p. 226), Professor F. H. Allport has elucidated and adroitly attacked several of the minor persistent problems in social psychology. The difficult nature of such a project becomes evident when one realizes that some psychologists[2] seem to doubt the existence of general patterns of facial expression corresponding to the various types of emotion. It is encouraging to bear in mind, however, that a comparatively greater number of theorists and investigators either have assumed that certain distinct patterns of facial expression may be reliably associated with specific emotional processes, or have presented experimental evidence to show that this is the case.[3] Whether each emotional state carries with it a definite configuration of facial features may still be a problem for conjecture, but it is almost a truism that observers react as if certain basic emotional processes were indicated by concomitant patterns of contraction in facial musculature. Most of us have gained at least a working proficiency in judging the emotional states of those about us by observation which largely concerns facial expression. Experimental evidence[4] indicates that certain primary emotional expressions are readily identified by most adults, while the finer nuances, especially in the attitudinal expressions (such as doubt, misgiving, mild disgust, or scorn) are less readily identified.[5]

Professor McDougall's theory of the "sympathetic induction of emotions" seems to imply that the ability to recognize emotional expressions, or at least to make differential responses to them, is innate.[6] The preponderance of opinion, however, seems to favor the view that this ability is partly or largely the result of training and experience. Murphy and Murphy (6, p. 70),[7] after reviewing the literature, have stated that the evidence is strong that there is no basis upon which an innate capacity to interpret emotional patterns can be predicted.

---

[2] Notably Carney Landis (5, pp. 497-498). Mandel Sherman (7) found that supposedly competent observers were unable to agree upon the correct identification of emotional responses in very young infants, the tendency being to name the emotion from the knowledge of the stimulus, rather than from observation of the response itself.

[3] For a review of the literature bearing on this and other problems in the study of facial expression see (4*a*).

[4] See footnote 3 above.

[5] It is the writer's belief, based upon experience in comparing the expressions of primitive peoples and those of cultures foreign to North America with those of persons in various parts of the United States, that certain basic emotional expressions are practically universal, but that the finer degrees of expression, which convey different meanings among various peoples, are matters of cultural modification. It is entirely possible that the specificity and unmodifiability of facial expressions vary directly with the amount or type of autonomic and visceral involvement in any emotional process.

Professor F. H. Allport (1, p. 226), finding that a wide range of ability to identify facial expressions existed among college students,[8] attributed these variations to differences of practice in reacting to the expressive criteria. Some persons, he suggested, have learned through special opportunity or incentive to read facial expressions, probably at an early age, while others, through lack of proper practice, have not acquired the ability to such a high degree. His conclusions are based on experiments with the facial expression test.[9] In the first of these experiments Allport found that fifteen minutes'[10] systematic training in analyzing facial expressions increased considerably the scores of those who originally stood low in the test, without increasing the scores of the better subjects. The gain of the poorer judges was thus roughly proportional to their lack of ability. This would seem to indicate that the ability to identify facial expressions was largely the result of experience and training, at the same time admitting the possibility of innate differences of a general sort in the sensitivity required to learn to interpret facial expressions. While the writer agrees for the most part with Allport's theories in this respect, he questions the efficacy of the experimental and statistical methods employed by Allport to support the theory. It is the purpose of this article to report several repetitions of the above-mentioned experiment, with a greater number of subjects, and to examine the validity of certain assumptions upon which Allport's conclusions have been based. Reference will also be made to a similar study conducted by Professor Guilford, in which the training was much more extensive than that afforded by Allport's procedure.

Allport (1, p. 209) gave the facial expression test to twelve young women. He then asked the subjects to study a chart of the synopsis of facial expressions for 15 minutes, after which the test was again administered. The effect of this brief training was quite pronounced. The average score at the first administration of the test was 48.8; after study of the chart it was 54.7, the average gain being 5.9. The coefficient of correlation by the rank difference method between the first standings and the amount of gain through study was —.86.[11] The results and conclusions from this experiment are succinctly stated by Professor Allport in the following passage from his *Social Psychology*:

---

[6] See Allport (1, p. 234).

[7] Experiments of C. Bühler, G. S. Gates, and F. H. Allport cited.

[8] Confirmed by the writer's more extensive data (4).

[9] Described in this journal (4).

[10] Murphy has erroneously reported this as "over a considerable period" (6, p. 69).

> "Judging from this table the following interesting facts and conclusions may be established: (1) out of twelve subjects, all but four improve in their identification of facial reactions with study and application of the principles of expression; (2) practice in this regard tends to equalize the ability of the various subjects. The less efficient gain the most, and the more efficient gain the least. The three best judges in the original test actually lost after study of the chart, and lost in direct proportion to their ability. The gain of the poorer judges was also roughly proportional to their *lack* of ability. There is, in other words, a fairly high inverse correlation (—.86) between the original ability to name expressions and the improvement through learning how to analyze the component positions of the features. (3) We conclude, therefore, that while there may be innate differences of a general sort in the sensitivity required to learn facial expressions, the broad differences between individuals in this respect are due to differences of practice in reacting to the expressive criteria. Some persons, through special incentive or opportunity, have already learned how to read faces, and probably at an early age. The *methods* by which they do it have become automatic and unconscious through continual use. To recall them, therefore, as in substituting conscious analysis, proves an unnecessary distraction and in some cases actually hinders their judgments, in the same way that playing with notes a pianoforte piece one has long played from memory usually confuses one. Others have never had the drive or the occasion to observe what emotions facial reactions indicate. The less they had noticed them the lower they stood in the test; hence their great improvement when this knowledge was acquired and used" (1, pp. 227-228).[12]

---

[11] Complete data from this experiment are recorded in (1, Table VII, p. 227).

[12] Quoted by permission.

The interpretation given by Professor Allport seems to have been generally accepted, though Professor Guilford (3, p. 194) has suggested that further tests and analyses of the factors involved should be made before Allport's conclusions are accepted as final. In analyzing the procedure, however, it seems to the writer that one may legitimately question whether 15 minutes of studying such a complicated chart as the one used actually results in a significant improvement in ability to name facial expressions. Fifteen minutes is a short period compared to a lifetime of experience in observing the behavior of other persons, probably with many opportunities for learning the names of patterns of emotion. An average gain of 5.9 points is shown in this experiment, but this is less than the gain in credit which would result if each subject were to improve by only so much as changing one incorrect response to a correct one.[13] Furthermore, the probable error of the average gain in this case is approximately 2.1, making the average gain less than three times its probable error.

VERIFICATION OF EARLIER RESULTS

Because the number of subjects in Allport's experiment was small, with a consequent limitation of statistical procedure, it seemed desirable to repeat and expand some of this earlier work, so as to answer more definitely such questions as the one proposed above.

Accordingly, the writer repeated the procedure described by Allport with 66 subjects (24 men and 42 women) who were students in either elementary or social psychology. The experiment with students in social psychology was performed in 1928; those in elementary psychology served as subjects about one year later. The procedure took place in classrooms where the students were accustomed to meet regularly. The size of the pictures, as projected on a screen by means of a stereoptican, was approximately 3x5 feet. After the first administration of the test, each student was given a mimeographed copy of the chart of the synopsis of facial expressions, with instructions to study it as best he could in order to improve his score in the future. At the expiration of 15 minutes, the charts were collected and the test was again administered.

---

[13] The test provides a possible range of scores from zero to 100. The scores actually recorded have ranged from 20 to 89, inclusive, the distribution approximating the "normal curve of probability."

TABLE 1
SUMMARY OF SCORES WHEN SLIDES WERE SHOWN TWICE WITH 15 MINUTES' STUDY OF CHART BETWEEN SHOWINGS
(66 subjects)

	I Before study	II After study	III Gain or loss
Mean of scores	48.9	55.4	6.8
P.E. of mean	±1.05	±1.06	±0.83
Median	50.3	55.7	6.3
P.E. of median	±1.31	±1.33	±1.04
S.D.	12.6	12.8	10.0
Range	55	52	48
Correlation Columns I and III: $r = -.38$, P.E. ±.07			

The data from this experiment are summarized in Table 1. In this table the mean of the scores before the study of the chart, 48.9, is almost identical with that found in Allport's experiment. The average amount of gain, 6.8, is slightly higher than the figure found by Allport (5.9), though when the probable errors are taken into account the difference is not significant. The probable error of the average gain, ±0.83, is sufficiently small to indicate that the increment is real, thus lending support to Allport's data.

EFFECT OF LENGTHENING THE STUDY PERIOD

As has been mentioned before, 15 minutes seemed a short time for learning the details of the chart. While no test was made to determine how much of the chart could be recalled by the subjects, the experimenter found himself unable to gain a working knowledge of the chart in 15 minutes, though his memory ability may have been below that of the subjects who took part in the experiment. In order to determine the effect of increasing the length of time devoted to study, a similar experiment was performed in which 29 students in elementary psychology were asked to study the chart for 45 minutes, instead of 15. With the exception of the time consumed, the conditions duplicated as nearly as possible those of the previous experiment. The results are summarized in Table 2. This table shows an average gain of 8.1 points with 45 minutes of study, which is 1.3 points higher than the average increment shown in Table 1. The probable error of the difference, ±1.4, is greater than the difference itself, however, so that the data from these two experiments afford no

TABLE 2
SUMMARY OF SCORES WHEN SLIDES WERE SHOWN TWICE WITH 45 MINUTES' STUDY OF CHART BETWEEN SHOWINGS
(29 subjects)

	I Before study	II After study	III Gain or loss
Mean	45.1	53.2	8.1
P.E. mean	±1.03	±1.28	±1.14
Median	44.5	55.3	7.8
P.E. median	±1.29	±1.60	±1.43
S.D.	8.3	10.3	9.1
Range	32	40	40
Average gain, Table 2,	8.1, P.E. ±1.1		
Average gain, Table 1,	6.8, P.E. ±0.8		
Difference	1.3, P.E. ±1.4		
Correlation Columns I and III: *r*= —.29 ±.11			

striking contrasts. Trebling the length of time for study seems to have little effect on the average gain as a result of training.

The net result of these experiments seems so far to be a confirmation of Allport's findings. Moreover, the number of subjects is large enough to reduce the probable errors of his figures considerably and thus to add to their reliability and validity.

The second result of the experiment to which Professor Allport calls attention in the paragraph previously quoted is that practice with the chart tends to equalize the ability of the various subjects.

> "The less efficient gain the most and the more efficient gain the least. . . . The gain of the poorer judges was also roughly proportional to their *lack* of ability. . . . There is, in other words, a fairly high inverse correlation between the original ability to name expressions and the improvement through learning how to analyze the component positions of the features."

The coefficients of correlation[14] shown in Tables 1 and 2 (—.38 and —.29, respectively) seem to warrant the same conclusion, except that these coefficients are numerically less than that found by Allport (—.86). Allport also mentioned that the three best judges in the original test actually lost in score after study of the chart. While such a condition is hardly paralleled in the present study, the same general tendency appears. In the first of our experiments, the data of which is summarized in Table 1, 12 of the 14 subjects who lost standing were above the mean on the first test. In the second experiment, 2 of the 4 losing subjects were originally above the mean.

---

[14] By the product-moment method. Allport used the rank-difference method.

A CONTROL EXPERIMENT

The above-mentioned figures, though not so convincing as the earlier data, seem to bear out Professor Allport's conclusions. In spite of this, however, the writer found himself tempted to persist in raising the question of whether these results were due to the study of the chart, or to other factors. The obvious course was to carry out a "control" experiment in which the previous conditions were duplicated so far as possible, except that the study of the chart was omitted, thus ruling out the element of training.

Fifty-seven students in elementary psychology, none of whom was familiar with the previous experiments, served as subjects. They filled out the test blanks as usual, after which they rested for 15 minutes. During this period the subjects were allowed freedom from classroom discipline, though they were requested not to discuss the pictures or the test. At the expiration of the rest period, the test was given a second time, with no explanation of the repetition.

The results of this control experiment are summarized in Table 3.

TABLE 3
SUMMARY OF SCORES WHEN SLIDES WERE SHOWN TWICE WITH 15 MINUTES' REST PERIOD BETWEEN SHOWINGS
(57 subjects)

	I First test	II Second test	III Gain or loss
Mean	47.2	46.4	—0.5
P.E. mean	±0.94	±0.77	±1.03
Median	45.9	47.1	0.5
P.E. median	±1.18	± .96	±1.29
S.D.	10.5	8.6	10.1
Range	46	34	46

Correlation Columns I and III:	$r=$ —.63, P.E. ±.05
Columns I and III, Table I:	$r=$ —.38, P.E. ±.07
Difference	—.25, P.E. ±.09

The data in this table are surprisingly similar to those previously obtained, except that on the average no gain resulted on the second administration of the test. Any average gain, such as appeared in the previous experiments, may therefore be attributed to the study of the chart itself and not to practice effects from the first testing. The coefficient of correlation between the standing of the subjects in the first test and the amount of gain or loss is —.63. A comparison between the coefficients with and without study, respectively, indicates that the latter is *higher* (negatively) by approximately three times the probable error of the difference. If the comparative positions of those who lost standing after the rest period are taken into account, it is discovered that 19 of the 27 who lost standing were above the mean on the first test.

Since this negative relationship between original standing and amount of gain or loss appears either *with* or *without* opportunity for study, to attribute it to the effect of training in the analysis of facial expressions would be at least gratuitous. One must therefore seek another pedigree for this phenomenon.

Importance of the Reliability of the Test

As an explanation of the fact that his three best judges lost standing after studying the chart, Professor Allport inferred that the loss was due to the distraction of substituting conscious analysis for automatic and unconscious methods of judgment. Yet this is not the only possible explanation of the facts. An alternative speculation is that the original high scorers were high *by chance,* and that a subsequent checking, by the same factor of "chance," tended to lower their standing. For example, if the names on the test blank were to be underlined entirely at random, with no understanding of the significance of the expressions, the probability is that in general the scores of those subjects who stood high in the distribution the first time would be lowered the second time; those who ranked at the lower end of the scale by a random checking might expect a second random checking to raise their scores for the most part. Thus the general tendency of "chance" would be to lower many of the original high scores and raise many of the original low scores. Granting that this were true, we should expect to find a negative correlation between the original standing of the subjects and the amount of "gain" or "loss."

It is perhaps obvious that the validity of Professor Allport's conclusions rests on the assumption that, in the absence of training, one might expect the standings of the subjects to be the same in two successive administrations of the facial expression test. In other words, he has assumed the reliability of the test to be perfect, a degree of reliability considerably beyond the usual standard of achievement. In effect, the foregoing mention of the factor of "chance" was to question the reliability of the test and to infer that the negative relationship between original standing and amount of gain or loss might be due to the unreliability of the test, rather than to the influence of study. If one may rely upon the usual methods of computing the reliability of a test, one should be able not only to determine the coefficient of reliability of the facial expression test, but also to predict the scores to be *expected* on the second administration of the test and to take these into account in drawing any final conclusions. If the test is not perfectly reliable, the true effect of the study of the chart can be shown only when the influence of the unreliability of the test has been ruled out. This the writer believes to have been accomplished by (*a*) finding the coefficient of reliability of the facial expression test by means of a retest after a considerable period of time; (*b*) using this coefficient to predict from the scores before study, by means of the regression equation, the scores which might be *expected* to occur on a second showing; (*c*) "partialling out" the effect of these latter scores on the coefficient of correlation between original standing in the test and amount of gain or loss after study. A possible weakness in this procedure is the assumption that the coefficient of reliability so found would hold for the group which studied the chart, but the writer sees no reason why this supposition is not valid. If it is correct, these data reveal no real justification for the conclusion that the less proficient subjects gain the most and the more proficient gain the least from the study of the chart.

To determine the reliability of the test, it was administered twice

TABLE 4
SUMMARY OF SCORES WHEN TEST WAS ADMINISTERED TWICE WITH THREE MONTHS' TIME INTERVENING

	I First test	II Retest
Mean of scores	45.8	46.0
P.E. mean	±.78	±.77
Median	45.9	45.0
P.E. median	±.98	±.96
S.D. (mean)	11.5	11.3

Correlation Columns I and II: $r = +.42$, P.E. ±.06

to 99 students in elementary psychology at the University of Nebraska, with a period of three months intervening. In the meantime, no mention was made in class of these tests or of the methods of identifying facial expressions. The data obtained by this procedure are summarized in Table 4.

The measures of central tendency are practically the same for the two administrations of the test. The coefficient of correlation between the scores on the first and second showings, respectively, was +.42.[15] This figure was taken to represent the coefficient of reliability of the test.[16] The standard deviation for the first test was 11.5, and for the retest, 11.3. Using these values, the regression equation in score form (2, p. 181) was solved for each of the scores made on the facial expression test before the 15-minute study period (i.e., the scores summarized in Column I, Table 1). The "net" correlation between (*a*) original standings in the facial expression test and (*b*) amount of gain or loss after study, with the influence of (*c*) the changes which might be expected on the basis of the known reliability of the test held constant, was —.066.[17] This would seem to be evidence that the negative correlation found between original standings and amount of gain or loss is due to the unreliability of the test, rather than to the effect of practice. Professor Allport's conclusions, based on this negative relationship arising from the erroneous assumption of the perfect reliability of the facial expression test, seem therefore to be contradicted by evidence from the present study. The writer does not wish to imply that Allport's theoretical explanations are unfounded, as they seem more than plausible, but only to point out that the present experimental evidence does not support the theory.

---

[15] The figure resulting from the correlation of the scores on the first half of the test with those of the second half and the application of Spearman's prophecy formula, using the data from 408 subjects who took the test for the first time, was +.24. [Formula from Garrett (2, p. 271).] The lowness of this figure is probably due to the fact that the first half of the test is less difficult than the second. The correlation of alternate items would undoubtedly have given a higher figure and would have done greater justice to the test.

[16] It is lower than the coefficient of correlation between the scores before and after the rest period, respectively, which was +.51 P.E. ±.07. The latter figure is not used in the present calculations, as it has been suggested that many of the changes made after the rest period were due to rivalry, desire to improve standing, and similar uncontrolled factors.

[17] Formula for $r_{12\cdot3}$, Garrett (2, p. 225).

EVIDENCE FROM PROFESSOR GUILFORD'S EXPERIMENT

An experiment reported by J. P. Guilford (3) would seem to support Allport's contention that the individuals who are poorer on the original test benefit most by training. Fifteen subjects took part in this experiment and the training was distributed over a period of 10 days. Four practically equivalent sets of 24 pictures each were selected from the 105 Rudolph pictures used by Langfeld. Each set contained slides representing the various expression groups as follows: pain-grief, 2; surprise-fear, 4; anger, 6; disgust, 4; pleasure, 2; attitudinal, 6.

Set I was given to the subjects at the beginning of the experiment to test their original ability and again at the end of the training period to measure the amount of improvement made. Between the first and second tests, the subjects were instructed to study, at their leisure, the anatomy of facial expression as presented in Allport's textbook (1, pp. 200-203). On the second day after the first test, the subjects viewed Set II of the slides and applied titles to them. Immediately after this the pictures of Set II were shown again, this time with the names of the expressions given. Following this instruction, the subjects were asked to study before the next class hour the material regarding the six groups of expression and the analytical features of each group (1, pp. 203-208). Test III was given on the second day following Test II. The pictures in Test III were repeated immediately, each picture being analyzed and discussed in the light of Allport's textbook. The subjects were then assigned the material on the synopsis of facial expressions as preparation for the next meeting of the class (1, pp. 208-210, especially the chart on p. 209). Test IV was given at the next meeting of the class, with special admonition to analyze the expressions carefully as they were shown on the screen. This time the slides were accompanied by their titles. The final assignment consisted of further material regarding facial expressions (1, pp. 210-221). The last test, with the slides of Set I, was given two days later, with the directions: "Judge by any method you please." In scoring the judgments, one point was allowed for classifying the expression in its proper group, one point for giving it the correct designation within the group and one point additional for each correct qualifying adjective.

On the first test the average percentage of correct scores was 27.2. On the final test the average improvement over the initial performance was 51 per cent. The variability among the subjects on the final test was quite small, according to Professor Guilford. Both the poorer and the better subjects approached the group average, so that the coefficient of correlation by the rank-difference method between original standing and amount of gain or loss was —.76±.07, a figure not far from Allport's —.86. This would seem to support Allport's contention admirably, particularly as the periods of training were long enough to allow for considerable learning.

It must be remembered, however, that in the 30-minute periods following Tests II, III, and IV the names of pictures quite similar to those in Test I were repeated and the subjects were given the opportunity and were encouraged to memorize the titles. Under the circumstances, it is not surprising to find that the poorer subjects improved considerably. Also, nothing in Guilford's study shows that the reliability of the first test was high. Certain subjects may have been high in the first place by chance, a possibility mentioned in criticism of Allport's experiment. In fact, Guilford reports that the two subjects who made no improvement over the original trial during the training period and who were among the leaders in Test I showed a decided loss in Test II.

It must also be borne in mind that Guilford's procedure included extensive drill in associating certain titles with specific types of picture—largely a memory process. Nothing in this experiment or in any of the experiments previously mentioned indicates without question that the subjects became any more able to recognize and name emotional expressions outside the laboratory than they were at the outset. The writer is not familiar with any literature regarding the transfer of training which would indicate that coaching in these particular situations would necessarily apply to the general situations of everyday life.

A further consideration is that not all of the pictures are recognized with equal ease. In Allport's test, for example, it was evident that some slides were correctly named more frequently than were others. Possibly the same was true of the pictures used by Guilford. An increase in score of seven points in Allport's test might mean that the subject had learned to recognize an easily identified slide, or it might indicate that he had become familiar with an exceedingly difficult expression. Guilford's procedure may have involved similar circumstances. The subjects who were low at the outset may have increased their standings by learning the easy expressions, while those who were originally high in the test could increase their scores only by learning the difficult ones. Possibly the actual increase in *ability* may have been about the same in all cases, even though the *scores* of the poorer subjects were increased more than those of the better subjects.

In summary, it seems reasonable to state that, so far as the significance of the negative correlation between original standing and amount of gain or loss is concerned, little evidence for the validity of Allport's interpretation has been added by Guilford's experiment.

In answer to Professor Allport's query as to how individuals differ in ability to name facial expressions, then, it has been shown that approximately the same differences mentioned by him are found to exist when large-scale methods are used. As for the factors to which these differences may be ascribed, however, it has been shown that conclusions should not be based on the negative correlation between original standing and amount of gain or loss after training, since a negative coefficient as high or higher may appear in the absence of training, and since the inverse relationship practically disappears when the effect of the unreliability of the test is ruled out.

Summary and Conclusions

1. The effect of training subjects for periods of 15 or 45 minutes in the analysis of facial expressions is a significant increase in the average of their scores on the facial expression test. This increase is due to training and not to practice gained in taking the test the first time.

2. Negative coefficients of correlation are found between the original standing of subjects in the facial expression test and the amount of gain (or loss) in test score after training in the analysis of facial expression, a circumstance which has previously been interpreted as indicating that the broad individual differences in ability to identify facial expressions of emotion are due in large part to differences of practice in reacting to expressive criteria.

3. An equally impressive negative correlation is found in a control experiment in which a rest period was substituted for the study period.

4. When allowance is made for the reliability of the facial expression test, the inverse relationship found in this study practically disappears. The negative coefficients between original standing and amount of gain (or loss) are therefore explained as consequences of the low reliability of the test, rather than as indications of the correctness of the theory advanced.

REFERENCES

1. ALLPORT, F. H. Social psychology. Boston: Houghton Mifflin, 1924. Pp. xiv+453.
2. GARRETT, H. E. Statistics in psychology and education. New York: Longmans, Green, 1926. Pp. 320.
3. GUILFORD, J. P. An experiment in learning to read facial expression. *J. Abn. & Soc. Psychol.,* 1929, **24**, 191-203.
4. JENNESS, A. Differences in the recognition of facial expression of emotion. *J. Gen. Psychol.,* 1932, **7**, 192-196.

4*a*. ————. The recognition of facial expressions of emotion. *Psychol. Bull.,* 1932, **29**, 324-350.

5. LANDIS, C. Studies of emotional reactions: II. General behavior and facial expression. *J. Comp. Psychol.,* 1924, **4**, 447-509.
6. MURPHY, G., & MURPHY, L. B. Experimental social psychology. New York: Harper, 1931. Pp. 709.
7. SHERMAN, M. The differentiation of emotional responses in infants: I. Judgments of emotional responses from motion picture views and from actual observation. *J. Comp. Psychol.,* 1927, **7**, 265-285.

University of Nebraska
Lincoln, Nebraska

LES EFFETS D'ENTRAÎNER DES SUJETS À RECONNAÎTRE DES EXPRESSIONS FACIALES

(Résumé)

On a rapporté une grande variation des différences individuelles dans la capacité d'identifier des expressions faciales de l'émotion. Comme des évidences que ces inégalités sont dues en grande partie aux différences de pratique dans la réaction aux critères expressifs, plutôt qu'aux facteurs innés, F. H. Allport a offert des données sur les effets d'entraîner des sujets de capacités différentes pendant quinze minutes dans l'analyse des expressions faciales. Les progrès faits par ses sujets dans le test des expressions faciales après la période d'entraînement ont été à peu près inversement proportionnels à leur capacité avant le test. En répétant ce procédé avec de plus grands groupes de sujets dans des périodes de la même durée et d'une plus longue durée, l'auteur a confirmé en général les données d'Allport. Cependant, une expérience de contrôle qui n'a permis aucune étude a montré des données semblables, sauf qu'en moyenne il n'a résulté aucuns progrès du résultat. On ne doit pas donc attribuer la relation inverse entre la capacité originale et la quantité de progrès ou de perte aux effets de l'entraînement. Une analyse plus étendue montre que quand on tient compte de la constance du test des expressions faciales le coefficient négatif disparaît à peu près. Le coefficient négatif, sur lequel Allport base sa théorie, résulte donc dans cette étude de la supposition erronée que la constance du test des expressions faciales est parfait. L'auteur, quoique disposé à croire à la théorie d'Allport, conclut que ce type d'évidence expérimentale ne soutient pas la théorie.

JENNESS

DIE EINWIRKUNG DER EINÜBUNG VON VERSUCHSPERSONEN IN DER ERKENNUNG VON GESICHTSAUSDRÜCKEN (FACIAL EXPRESSION)

(Referat)

Es werden grosse individuelle Unterschiede gemeldet in der Fähigkeit, Gesichtsausdrücke richtig zu erkennen. Als Beweis, dass diese Ungleichheiten grossentiels auf Verschiedenheiten der Einübung im Reagieren auf Ausdruckskriterien und relativ wenig auf angeborene Eigenschaften der Versuchspersonen ruhen hat F. Allport statistisches Material dargeboten über die Einwirkung der Einübung von Versuchspersonen von verschiedener Geschicklichkeit in der Analyse von Gesichtsausdrücken. Die von seinen Versuchspersonen errungene Erhöhung der an der Ausdrucksunterscheidungsprüfung (facial expression test) erhaltenen Zahlen nach der Einübungsperiode stand ungefähr in umgekehrtem Verhältniss (inverse relation) zu dem ursprünglichen statistischen Rang der Versuchsperson. Allport's Befunde wurden im Allgemeinen von dem gegenwärtigen Verfasser an grösseren Gruppen von Versuchspersonen sowohl für die selbe Lernperiode wie auch für längere Lernperioden bestätigt. Ein Kontrollversuch aber, in dem keine Gelegenheit zum Lernen gegeben wurde, ergab ähnliche Befunde, mit der Ausnahme, dass im Durchschnitt keine Erhöhung der Zahl (score) stattfand. Das umgekehrte Verhältniss zwischen der ursprünglichen Geschicklichkeit darf daher nicht der Einwirkung der Einübung zugeschrieben werden. Eine weitere Analyse zeigt, dass wenn man die Zuverlässigkeit der Ausdrucksunterscheidungsprüfung (facial expression test) in Beziehung nimmt, der negative Koeffizient fast verschwindet. Es zeigt scih also, dass der negative Koeffizient, worauf Allport seine Theorie gründet, seinen Ursprung in der falschen Annahme hat, dass die Zuverlässigkeit der Ausdrucksunterschiedungsprüfung vollkommen ist. Der gegenwärtige Verfasser, obwohl er geneigt ist, der Allport'schen Theorie beizustimmen, unrteilt, dass diese Art von experimentellem Beweis die Theorie nicht unterstützt.

Jenness

SHORT ARTICLES AND NOTES

EDITED BY
F. C. BARTLETT
V. BOROVSKI
F. KIESOW
MATATARO MATSUMOTO
WALTER MILES
R. W. WILCOCKS

A CLASSIFICATION FOR PSYCHOLOGY

RAYMOND ROYCE WILLOUGHBY

The scheme here presented was elaborated in the early part of 1930 in connection with a proposed fundamental revision of the categories used in the *Psychological Index;* it was filed when the decision was made to revise the *Index* scheme only superficially. It is here resurrected in view of the action of the 1931 meeting of the American Psychological Association on the suggestion that it endorse the Dewey system for psychology, and in view of the distribution for comment of Dr. C. M. Louttit's excellent decimal system.

If there is any merit in the present scheme, it inheres principally in the fact that it was elaborated upon the basis of actual material requiring classification (*Index* entries). It is customary, though probably not especially penetrating, to speak of classifications in terms of the antithesis "logical-practical" or some equivalent, the implication being that the speaker's viewpoint, by virtue of his venerable experience, is "practical" as against the "merely logical" outlook of his more callow and academic opponent. The present writer, however, has consistently failed to detect the essential difference between the two points of view, and so has endeavored to incorporate both in the scheme. The numbers in parentheses may be regarded as lying upon the "practical" side of the continuum; they represent the approximate volume of material to be classified, in numbers of entries; while attempting to build a logically fairly consistent structure, it has been attempted also to achieve a result in which the smallest categories should be neither unwieldy, very small, nor seriously disproportionate to each other. This has also furnished a criterion for the extent of the subdivision. The classificatory equivalence of the taxonomically very different groups 112, *Invertebrates,* 114, *Rats,* and 115, *Other rodents,* is based upon this principle.

Logically, any classification reduces to an attempt to represent a multidimensional continuum in terms of a linear one without too greatly distorting habitual ways of thinking about the material; it is self-evident that this can rarely be done with anything like complete satisfaction. It is probable also that any attempt to achieve smoothness, i.e., to make the linear, representational continuum really a continuum, involves the imposition of at least one systematic point of view, upon which, of course, other thinkers are free to disagree and hence to reject the classification. The principle governing the smoothing of the continuum in the present scheme is that of growth from biological to social; the writer feels no particular embarrassment in admitting that he cannot say rigorously what he means by these terms, or by any other similar ones in this region; he may indicate his meaning, however, by citing such a continuum as reflex-emotion-attention-speech-education as one progressing in the same direction as the classification offered.

The problem of general material cuts across the principle just described to some extent; this has been solved arbitrarily in the manner suggested by Dewey, namely, by placing general material before specific. The problem of adjusting the last part of one section to the first part of the section following it has been handled in the manner adopted by biological taxonomists (with whose problem, in fact, the general one under examination has very many correspondences), viz., by placing the primary divisions in the order required by the organizing principle, and the subdivisions in the same order within the primary divisions, without worrying too much about the fact that in such an arrangement the highest order of the monocotyledons (for example) is universally conceded to be "higher" than the lowest order of the dicotyledons, which follows it. A striking example of this "unconformity" is afforded by 145, *Problems of adjustment* (of the child), which is "higher," of course, than, e.g., 25, *Vegetative functions,* but owes its position to its subordination to 1, *Genetic psychology.*

The decimal scheme has been retained; it is hard to imagine any serious competitor to it in convenience and flexibility. It will be observed that there are a quite sufficient number of places vacant to permit a large degree of expansion. The decimal point has been dropped, however; it serves no useful purpose in any scheme in which the number of digits is within the span of a single fixation.

The complete classification follows:

0 General considerations
  00 General expositions (50)
  01 History of psychology
    010 General history (30)
    011 Individuals (35)
    012 Congresses (25)
  02 External systematization
    020 Psychology and philosophy (35)

        - 021 Psychology and other sciences (25)
    - 03 Internal systematization
        - 030 General (20)
        - 031 Subjective psychologies (15)
        - 032 Objective psychologies (20)
        - 033 Gestalt (20)
    - 04 General methods
        - 040 Statistical methods (35)
        - 041 General apparatus (35)
    - 05 Bibliographies, collections, journals (35)
- **1 Genetic psychology**
    - 10 General; mind in evolution (16)
    - 11 Sub-human forms
        - 110 General comparative psychology (40)
        - 111 Plants (3)
        - 112 Invertebrates (35)
        - 113 Fish, amphibians, reptiles, birds (35)
        - 114 Rats (55)
        - 115 Other rodents (40)
        - 116 Primates (40)
        - 117 Other mammals (50)
    - 12 Inheritance
        - 120 Genetic mechanisms (35)
        - 121 Population phenomena (50)
        - 122 Eugenics (35)
    - 13 Differential psychology
        - 130 Race differences (45)
        - 131 Sex differences (25)
        - 132 Individual differences (25)
        - 133 Group and occupational differences (15)
    - 14 Childhood and adolescence
        - 140 General child psychology (55)
        - 141 Physical characteristics (35)
        - 142 Intellectual characteristics (55)
        - 143 Personality characteristics (50)
        - 144 Special child types (15)
        - 145 Problems of adjustment (80)
    - 15 Primitive mentality
        - 150 General (15)
        - 151 Custom (40)
        - 152 Superstition and folk-lore (30)
    - 16 Instinct (10)
- **2 Psychophysical correlations**
    - 20 Constitution (15)

- 21 Brain localization (20)
- 22 Reflexes
  - 220 Handedness and eyedness (10)
  - 221 Galvanic reflex (20)
  - 222 Eye reflexes (40)
  - 223 Knee jerk (15)
  - 224 Conditioned reflex (25)
  - 225 Posture, tonus, contraction (30)
  - 226 Other reflex phenomena (25)
- 23 Glandular phenomena (35)
- 24 Drugs (20)
- 25 Vegetative functions
  - 250 General (20)
  - 251 Specific (50)
- 26 Voluntary motor phenomena (45)
- 27 Neurology
  - 270 Structure (35)
  - 271 Function (30)

3 Depth psychology

- 30 Psychoanalysis
  - 300 Techniques; general psychoanalysis (65)
  - 301 Applied psychoanalysis (50)
  - 302 Genesis of personality (50)
  - 303 Cases and symptoms (40)
- 31 Dreams, hallucinations, imagery (35)
- 32 Occultism (60)
- 33 Emotions
  - 330 General theory of emotions (60)
  - 331 Expression of emotion (30)
  - 332 Feeling and pleasantness (25)
  - 333 Specific emotions (25)
- 34 Hypnosis and suggestion (25)
- 35 Esthetics and art
  - 350 General theory (25)
  - 351 Visual art (25)
  - 352 Music (45)
  - 353 Literature (15)
- 36 Religion
  - 360 General and formal aspects (60)
  - 361 Social aspects (30)
  - 362 Religious experience (50)
- 37 Character
  - 370 Qualitative studies (50)
  - 371 Character estimation (40)

- **4 Disorders**
  - 40 General considerations
    - 400 General theory (40)
    - 401 Psychotherapeutic methods (35)
    - 402 Organization (25)
    - 403 Mental hygiene (25)
  - 41 Organic relationships
    - 410 Encephalitis (40)
    - 411 Paresis (20)
    - 412 Brain tumor (20)
    - 413 Other organic disorders (23)
  - 42 Cyclothymia (15)
  - 43 Schizophrenia (50)
  - 44 Psychoneurosis (50)
  - 45 Epilepsy (25)
  - 46 Delusions, interpretations, paranoia (30)
  - 47 Motor disturbances (35)
  - 48 Feeble-mindedness (55)
- **5 Sensation and perception**
  - 50 General
    - 500 Psychophysical methods (3)
    - 501 General sensory phenomena (35)
    - 502 General sensory impairment (10)
  - 51 Vision
    - 510 General; optic principles (20)
    - 511 Color (50)
    - 512 Binocularity; space perception (30)
    - 513 Brightness, illumination, adaptation (40)
    - 514 Thresholds and acuity (15)
    - 515 Images, illusions, configuration (55)
    - 516 Seen movement (40)
    - 517 Visual impairment (30)
  - 52 Audition (45)
  - 53 Smell and taste (3)
  - 54 Cutaneous senses (20)
  - 55 Motor and static senses (10)
  - 56 Time sense (20)
- **6 Higher mental processes**
  - 60 Intelligence
    - 600 Nature and constancy (40)
    - 601 Test theory and methods (40)
    - 602 Specific tests (40)
  - 61 Will; mental work (15)
  - 62 Attention (30)

- 63 Learning
  - 630 Habit (15)
  - 631 Ideational learning (50)
  - 632 Memory (60)
- 64 Thought (50)
- 65 Testimony and belief (20)

7 Social psychology

- 70 General
  - 700 General social theory (50)
  - 701 Elementary social processes (10)
  - 702 Social investigation techniques (15)
  - 703 Analysis of cultures (10)
- 71 Language
  - 710 Significance of language (5)
  - 711 Meaning (20)
  - 712 Gesture (3)
  - 713 Speech (55)
  - 714 Writing (15)
  - 715 Reading (20)
- 72 Sex
  - 720 Elementary sexual phenomena (25)
  - 721 Socialized sexual phenomena (40)
- 73 Crime
  - 730 Inadequacy and adjustment (10)
  - 731 Genesis of crime (5)
  - 732 Psychiatry of delinquents (45)
- 74 Education
  - 740 Significance and theory (30)
  - 741 Experimentation (30)
  - 742 Teachers (30)
  - 743 Scholastic adaptation (75)
  - 744 Vocational guidance (75)
  - 745 Moral education (60)
  - 746 Special education (40)
  - 747 Specific subjects (105)
  - 748 Measurements (65)
  - 749 General study techniques (35)
- 75 Technopsychology
  - 750 General theory (20)
  - 751 Occupational selection (55)
  - 752 Occupational training (10)
  - 753 Conditions of work (40)
  - 754 Cooperation and morale (3)
  - 755 Performance and fatigue (15)

    756 Advertising and selling (10)
    757 Specific vocations (10)
  76 Ethics (25)
  77 Group interaction (30)

Clark University
Worcester, Massachusetts

FOVEAL SENSITIVITY AS INFLUENCED BY PERIPHERAL STIMULATION

Frank A. Geldard

Earlier papers by the writer (3, 4, 5) have served to demonstrate that the sensitivity of a given local area of the retina is not solely a function of the nature and intensity of the stimulus affecting it; local sensitivity has a very definite dependence upon conditions of stimulation in neighboring areas. This interaction effect has been investigated not only for the human eye (5, 6) but also in preparations in which lateral retinal connections are relatively more abundant. The noteworthy series of experiments performed by Adrian and Matthews (1) has quite decisively demonstrated the neural basis for independence of retinal areas.

A relation which has yielded itself to precise quantitative statement is that obtaining between the level of foveal sensitivity and the intensity of a peripheral stimulus. When white light is used in both regions, it has been shown that a peripheral stimulus serves in the capacity of a facilitating agent to increase the sensitivity of the fovea. Furthermore, the extent of the increase has been shown to be a logarithmic function of the intensity of the peripheral light (5).

Since the completion of the work establishing this relation, which may be taken to be a statement of the quantitative aspect of brightness contrast, there has appeared the report of experiments, performed by R. Granit (6), designed to test the extent of interaction within the fovea and within a peripheral area. Granit, taking as a point of departure the method used by Adrian and Matthews, employed the critical frequency of flicker as a measure of sensitivity. Under conditions of multiple peripheral stimulation, marked facilitation effects were noted. Transferring the same light stimuli to the foveal region, no appreciable interaction could be found. While these results are not directly at variance with those previously obtained by the writer, other considerations prompted the belief that the failure to find foveal facilitation might be due to the use of the flicker-point method. There existed the possibility that the employment of intermittent stimuli, particularly at low intensities, prevented the realization of the normal facilitation effect.

Accordingly, two experiments were designed which would answer the question as to whether interaction is dependent upon continuous stimulation and which would, at the same time, incorporate features that would reduce the rather gross variability found in previous flicker experiments. The first experiment made use of a foveal stimulus of constant brightness (3.45 ml.) and of sensation daylight quality. This light subtended a visual angle of 31′. A light of variable brightness, the greatest intensity being nearly 2000 times the lowest, was thrown into a parafoveal area at a distance subtending a visual angle of 5 degrees from the point of fixation, the latter being the center of the foveal light spot. A sectored disc was rotated in the path of the foveal light, the disc being mounted on the shaft of a small, variable-speed, oscillator-driven synchronous motor, developed in the Virginia laboratory for this purpose. An article describing the construction and operation of the motor has already appeared (2). The flicker frequency was varied in equal steps and the critical flicker point was determined by the psychophysical Method of Limits. All observations were monocular and were made with an adequately registered and confined artificial pupil in the optical path. For one observer the following conditions of illumination were used: (1) foveal stimulus alone = 3.45 ml., (2) peripheral stimulus, hereafter designated for convenience "light B" = .67 ml., (3) B = 9.99 ml., (4) B = 136.17 ml., (5) B = 1247.33 ml. For each of these situations the critical frequency of flicker of the foveal light was determined. As shown by Table 1 the flicker points, expressed in flashes per second (the light and dark intervals being equal), show a steady rise with increasing intensity of the peripheral stimulus. The central light was, of course, maintained at a physically constant intensity throughout the experiment.

TABLE 1

	Critical frequency (flashes/sec.)
Foveal stimulus alone	17.75
Peripheral stimulus = .67 ml.	19.06
Peripheral stimulus = 9.99 ml.	19.56
Peripheral stimulus = 136.17 ml.	19.88
Peripheral stimulus = 1247.33 ml.	20.50

It is interesting to note that the sensitizing effect is apparent phenomenally. Observers used in the preliminary trials, who were unaware of the stimulus conditions, were able to report that an appreciable increase in brightness of the foveal stimulus accompanied the introduction of the peripheral stimulus.

The results reveal an obvious relationship when expressed graphically. If the critical frequency of flicker, in flashes per second, is plotted against

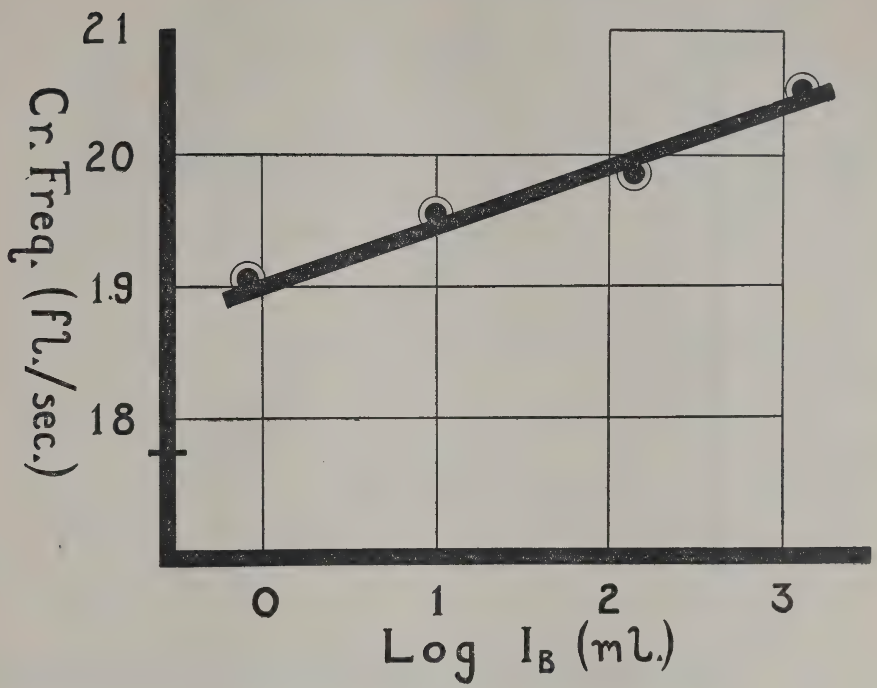


FIGURE 1

the logarithm of the intensity of the peripheral stimulus, a linear relation is seen to obtain (Figure 1). The formula fitting the curve has the form:

$$F = K \log I_B + C$$

where F is the critical frequency of flicker, I_B is the intensity of the peripheral stimulus, and K and C are constants. For these data the slope constant, K, has a value of .457 while $C = 19.0$ Thus the logarithmic relation found in the earlier work again proves to express the essential facts of retinal interaction. Here, of course, the slope constant is reversed in sign; retinal sensitivity and critical flicker frequency are related positively.

The second experiment was designed as a check upon the first and was carried out concurrently.[1] The procedure here was the complement of that outlined above. The frequency of flicker was held constant at 24 flashes per second, while the intensity of the foveal stimulus was varied in

---

[1] My thanks are due Mr. J. M. Porter, Jr., for serving as experimenter in this part of the work.

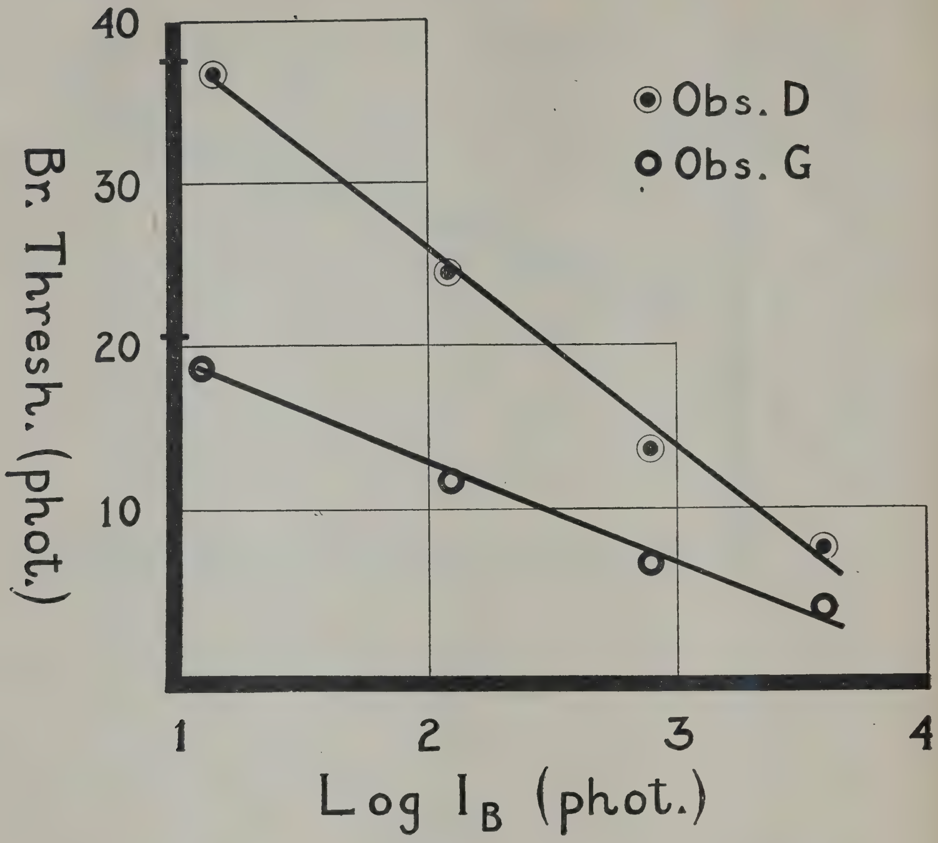


FIGURE 2

small steps. Here again the Method of Limits was employed, this time to determine the brightness necessary to just extinguish flicker. The same conditions of peripheral stimulation were present, brightnesses of .67 ml., 9.99 ml., 136.17 ml., and 1247.33 ml. being used. No artificial control was imposed upon the pupil but the brightness measures were later corrected with the aid of Reeves' data (7) to yield values expressing brightness of the retinal image.

The results for two observers are expressed graphically in Figure 2. The thresholds of flicker (in photons) plotted against the logarithms of the brightnesses of peripheral stimuli (in photons) again reveal the straight-line relationship. Here, since sensitivity is being measured in terms of brightness thresholds, the slope constant, *K*, is negative. This experiment proves a suitable check upon the former one, while both adequately substantiate the expression of brightness contrast derived from the earlier work.

The answer to our query now seems obvious. The critical frequency of flicker presents no unusual problems in the measurement of retinal sensitivity. If the concept be allowed, it may be thought of as a measure of "temporal visual acuity" as contrasted with "spatial visual acuity" which is ordinarily measured directly in terms of threshold brightness or separation of contours.

REFERENCES

1. ADRIAN, E. D., & MATTHEWS, R. The action of light on the eye. III. The interaction of retinal neurones. *J. Physiol.,* 1928, **65,** 273-298.
2. DAVIS, R. C., & GELDARD, F. A. An oscillator and synchronous motor for obtaining exact variable speeds. *Science,* 1931, **73,** 369-370.
3. GELDARD, F. A. The measurement of retinal fatigue to achromatic stimulation. I. *J. Gen. Psychol.,* 1928, **1,** 123-135.
4. ————. The measurement of retinal fatigue to achromatic stimulation. II. *J. Gen. Psychol.,* 1928, **1,** 578-590.
5. ————. Brightness contrast and Heymans' Law. *J. Gen. Psychol.,* 1931, **5,** 191-206.
6. GRANIT, R. Comparative studies on the peripheral and central retina. I. On interaction between distant areas in the human eye. *Amer. J. Physiol.,* 1930, **94,** 41-50.
7. REEVES, P. Rate of pupillary dilation and contraction. *Psychol. Rev.,* 1918, **25,** 330-340.

University of Virginia
University, Virginia

THE MEASUREMENT OF RETINAL ILLUMINATION

D. McL. PURDY

It has been pointed out by Troland (9) that the adequate specification of a visual stimulus, for the purposes of physiological optics, involves a statement of the actual intensity of illumination on the retina. This retinal illumination is proportional not only to the external stimulus brightness, but to the area of the pupil. The size of the pupil is not constant, but diminishes with increasing intensity of stimulation. In order to obtain a measure of retinal illumination, therefore, we need either to know the size of the natural pupil under the given conditions, or else to employ in our observations an artificial pupil of known size which is always smaller than the natural pupil.

The definition of a unit of retinal illumination must specify a standard value both for the external stimulus and for the pupillary area. König was apparently the first to introduce such a unit. The "König unit" is defined as that intensity of retinal illumination which is obtained when a magnesium oxide screen normally illuminated by one-tenth of a Violle standard at one meter's distance is viewed through an artificial pupil 1 sq. mm. in area (8). In more recent times, a unit invented by Troland (9), the "photon," has found rather extensive use in visual research. This unit is defined as that intensity of retinal illumination which accompanies the use of an external stimulus brightness of one candle per sq. m. and an artificial pupil of 1 sq. mm.

In view of the importance of much of König's quantitative work, it is of much interest to know the relation which his unit bears to the one currently employed. According to Schroeder (8), 1 photon=2.1 König units (with an uncertainty of 10%).

In the work of certain recent writers (3, 4, 5), one finds the word "photon" used in a different sense from that of Troland, namely, to designate a unit defined in terms of the millilambert, rather than the candle per square meter, as the standard stimulus brightness. This new unit, which is equal to $10/\pi$, or 3.18, of Troland's units, has a certain advantage, from the point of view of ease of computation. However, for the sake of avoiding confusion, it is to be strongly recommended that some new name be found for this unit if its use is to be continued.

In the older work, aside from that of König, Ives, and Nutting, an artificial pupil was seldom employed, and where light intensities were measured one knows, at the best, only their photometric values. If, however, the sizes of the natural pupil at the various intensities were known, it would be possible to calculate the actual retinal illuminations corresponding to the photometric values. Such a reduction of the results would be of great value for comparative purposes.

A number of experimenters have studied the relation between stimulus brightness and pupillary diameter, perhaps the best of these investigations being that of Reeves (7).[1] The question arises whether, given known brightnesses, we cannot simply make use of these standard pupil measurements and calculate the corresponding retinal stimulus values. As a matter of fact, this procedure has been followed by numerous writers.

Assuming the validity of this method, the present writer had in mind the preparation of a table which would permit the immediate translation of photometric brightnesses, viewed with the natural pupil, into photon values. Further consideration, however, has led him to believe that such calculations would be quite unreliable.

---

[1] It should be noted, however, that the recent data of Schroeder and of Couvreux (8) are in pronounced disagreement with those of Reeves. There is no obvious explanation for this discrepancy.

The fact that, as Reeves' data show, there are individual differences in pupillary size under the same illumination, constitutes one objection. But, for most purposes of physiological optics, these differences are not very serious. The average data for Reeves' subjects would probably give a sufficient approximation to the truth.

We must bear in mind, however, that in the major part of Reeves' work binocular stimulation was employed. But Reeves did perform some experiments with monocular stimulation, and he found the pupil to be considerably larger than when both eyes were exposed to the light. In our calculations, therefore, we should have to take account of the mode of stimulation employed, and apply the corresponding pupillary data.

But the diameter of the pupil varies not only with the light intensity, but with the accommodation of the eye. In Reeves' experiments, the fixation point was 35 cm. from the eye. We cannot legitimately apply his data to a situation in which the eye was not accommodated for approximately the same distance. According to the experiments of Couvreux (8), a change from 0 to 3 diopters in the accommodation may cause the pupillary diameter to change by as much as 100%.

But, even with the same stimulus brightness, the same adaptation, and the same accommodation, the pupillary diameter may not correspond to the data of Reeves. In his experiments the eye was exposed to a surface 60 cm. square.[2] The visual angle subtended by this stimulus was about 80°. Can we assume that with a very much smaller stimulus area, such as ordinarily figures in visual experiments, the pupil will have the same diameter? Is the pupil size determined merely by the brightness of the brightest part of the visual field—or does it correspond more nearly to the *average* brightness of the whole field? The increased size of the pupil in monocular, as compared with binocular, stimulation indicates that the pupil does tend towards a size approximating the average illumination of the two retinae. We might expect that it would obey a similar law with respect to the single retina.

Some simple but ingenious experiments performed by Mach (6) indicate that this is in fact the case, and that the pupillary size depends upon the total quantity of light striking the retina, and not simply upon the maximal brightness in the field.[3] In our present situation, therefore, the area of stimulation will play a part. Now, if we were certain that the pupillary size is fixed in all cases by the total luminous flux, without any regard to its distribution, our difficulty could be overcome. After calculating the actual quantities of light received by the eye in Reeves' experiments, we could plot a curve representing pupillary size as a function of this quantity.

---

[2] The size of the exposure surface is not given in the original paper; the writer has obtained this information through the courtesy of Professor Prentice Reeves and Mr. Lloyd A. Jones.

[3] Cf. also Abelsdorff and Feilchenfeld (1).

For a stimulus of known brightness and area, the corresponding pupillary size could be obtained from this curve.

However, it is highly unlikely, in view of the different pupillomotor sensitivities of the different retinal regions, that such a simple law obtains. We are forced to conclude, then, that the accurate calculation of retinal illuminations is impossible unless the pupil is actually measured under the given experimental conditions, or unless we adopt the more convenient method of using an artificial pupil, or its equivalent (2), of known, constant aperture.

References

1. Abelsdorff, G. & Feilchenfeld, H. Ueber die Abhängigkeit der Pupillarreaktion von Ort und Ausdehnung der gereizten Netzhautfläche. *Zsch. f. Psychol.*, 1904, **34**, 111-131.
2. Ferree, C. E., & Rand, G. A substitute for an artificial pupil. *Psychol. Rev.*, 1916, **23**, 380-382.
3. Geldard, F. A. Brightness contrast and Heymans' law. *J. Gen. Psychol.*, 1931, **5**, 191-206.
4. Hecht, S. The relation between visual acuity and illumination. *J. Gen. Physiol.*, 1928, **11**, 255-282.
5. Houstoun, R. A., & Shearer, J. F. Weber's law and visual acuity. *Phil. Mag.*, 1930, **10**, 433.
6. Mach, E. Ueber den physiologischen Effekt räumlich vertheilter Lichtreize. *Sitzber. d. Akad. d. Wiss. zu Wien, math.-naturwiss. Kl.*, 1865, **52** (2), 131.
7. Reeves, P. The rate of pupillary dilation and contraction. *Psychol. Rev.*, 1918, **25**, 330-340.
8. Schroeder, H. Die zahlenmässige Beziehung zwischen den physikalischen und physiologischen Helligkeitseinheiten und die Pupillenweite bei verschiedener Helligkeit. *Zsch. f. Sinnesphysiol.*, 1926, **57**, 195-223.
9. Troland, L. T. On the measurement of visual stimulation intensities. *J. Exper. Psychol.*, 1917, **2**, 1-33.

University of Kansas
Lawrence, Kansas

DIFFERENCES IN THE RECOGNITION OF FACIAL EXPRESSION OF EMOTION

Arthur Jenness

Are women more adept at recognizing facial expressions of emotion than are men? Which patterns of facial expression of emotion are most readily and correctly indentified? Considerable disagreement in the answers to these questions is to be found in the literature concerning the recognition of facial expression of emotion.

F. H. Allport (1, p. 226) and J. P. Guilford (6) have reported that there seem to be no pronounced sex differences among college students in the ability to recognize facial expressions of emotion as portrayed by the Rudolph pictures. G. S. Gates (5) found that among children from 6 to 14 years of age neither sex excelled the other significantly in naming the expressions portrayed by certain of Ruckmick's pictures. On the other hand, D. E. Buzby (2) has reported that, with certain of the Titchener and Boring model faces, women gave a slightly higher percentage of correct judgments than did men.

At the risk of further complicating the issue, the writer offers his data from the administration of Allport's facial expression test[1] to 536 college

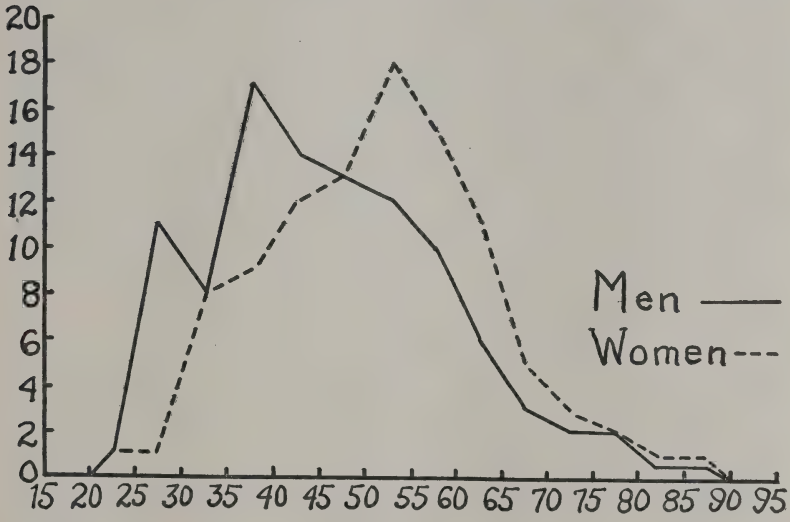


FIGURE 1
Distribution of Scores on Facial Expression Test
(According to sex)

Measure	Men	Women
Number of subjects	196	340
Mean of scores	46.12	50.78
P.E. mean	0.55	0.32
Median of scores	44.70	51.17
P.E. median	0.69	0.40
Range of scores	70	70
Quartile deviation	9.11	8.41
Standard deviation	11.35	8.70

---

[1] As yet unpublished.

students. The pictures used in this test are 14 of the Rudolph series, previously selected by Professor Langfeld. Each subject is given a sheet containing 14 groups of names of expressions. Each of these groups comprises 8 titles, several of which approximate the correct title of the corresponding picture, but only one of which is wholly correct. The pictures are shown on a screen for one minute each by means of a stereoptican, the projections being 3 by 5 feet in extent. The subjects are asked to underline the *one* title in each group which seems to them most nearly to describe the corresponding expression. Partial credit is allowed for designations which are not entirely correct. The possible range of scores is from 0 to 100; the lowest actual score so far reported being 20, the highest, 89.

The 196 men and 340 women whose performances furnished the data summarized in Figure 1 were students in introductory or social psychology at either Syracuse University, the University of California at Los Angeles,[2] or the University of Nebraska. In Figure 1 the means and medians for the women are slightly higher than those for the men, the sex differences in both cases being more than seven times the probable error of the difference. These disparities, though small, are significant, particularly as the curve for the men is noticeably positively skewed, in contrast to the curve for the women. In interpreting these sex differences, however, one must remember that in this test the pictures portray only masculine features. It is quite possible that adding female faces to the test might alter the distribution of scores.

Relative Identifiability of Expressions

H. S. Langfeld (7), using 105 of the Rudolph pictures with six subjects, found that laughter seemed to be most easily and correctly identified, the other expressions, from least to most difficult, being amazement, bodily pain, hate (aversion), fear, disgust, doubt, anger.

Allport (1, p. 224), in administering the facial expression test to 48 students, also found that laughter was most readily identified, with bodily pain, fear (horror), distrust (hate), amazement, anger, doubt, and disgust following in order, the last named being least frequently recognized.

C. A. Ruckmick (8, pp. 30-35) found that in naming certain facial expressions from photographs of an experienced amateur actress, the observers were more uniform in naming the primary emotions, such as love, hate, joy, and sorrow, than they were in naming the secondary emotions, such as repulsiveness, surprise, distrust, and defiance. G. S. Gates (5) showed six of the Ruckmick pictures to 458 children ranging from 6 to 14 years of age. The order of difficulty of identification, from least to greatest, was: laughter, pain, anger, fear, surprise, scorn. The order when the same pictures were used with adults was: laughter, scorn, fear, anger, pain, surprise.

---

[2] The writer is indebted to Professor F. H. Allport for the data on this group of 130 students.

Buzby (2) found that when students were asked to check the correct titles for certain of the facial expressions as portrayed by the Titchener and Boring model the percentage of correct judgments was high for the horrified, disdainful, disgusted, and bewildered faces, respectively, and low for the anger and dismayed expressions. Ellen Jarden and S. W. Fernberger (3), in a later article, agree with Buzby's conclusions in this regard.

J. Frois-Wittmann (4) carried on an extensive investigation which bears on the problem of the relative identifiability of various patterns of facial expression of emotion, but the writer finds himself unable to summarize Frois-Wittmann's findings briefly.

In the writer's investigation, the data from the administration of Allport's facial expression test to 256 subjects was analyzed to discover which of the expressions were most frequently named correctly. Where the type of expression shown in the lists of Langfeld and Allport was represented by only one slide, the number of times the correct title was underlined was used as the basis for ranking that pattern. Where more than one slide portrayed the same type of expression, the average of the frequencies with which the correct titles were underlined was used to determine the rank. The results are summarized in Table 1.

TABLE 1

FACIAL EXPRESSIONS IN ORDER OF IDENTIFIABILITY
Data from Facial Expression Test—256 Subjects

Emotion group	Slide number	Average number times underlined
Amazement	10	132
Laughter	1, 5, 9	127
Bodily pain	2, 13	126.5
Anger	11, 4	109.5
Distrust	14	96
Disgust (scorn)	3, 6, 8	65.3
Fear (horror)	7	63
Doubt (misgiving)	12	11

The suggestion might be made here that generalizations regarding the identifiability of different patterns of facial expressions are hardly justified if (as in the present case) they are based on the use of only a few pictures or on only a few types of expression. The differences in identification in this study may have been due to the fact that some slides show more clear-cut expressions than others, or to the fact that certain slides represent better portrayals of emotion. This criticism might also apply to the faces of the Titchener and Boring model. Langfeld, on the other hand, used a large number of slides, but the number of subjects taking part in his experiment was too small to allow for wide generalization from his data.

References

1. Allport, F. H. Social psychology. Boston: Houghton Mifflin, 1924. Pp. xiv+453.
2. Buzby, D. E. The interpretation of facial expression. *Amer. J. Psychol.,* 1924, **35**, 602-604.
3. Fernberger, S. W., & Jarden, E. The effect of suggestion on the judgment of facial expression of emotion. *Amer. J. Psychol.,* 1926, **37**, 565-570.
4. Frois-Wittmann, J. The judgment of facial expression. *J. Exper. Psychol.,* 1930, **13**, 113-151.
5. Gates, G. S. An experimental study of the growth of social perception. *J. Educ. Psychol.,* 1923, **14**, 449-461.
6. Guilford, J. P. An experiment in learning to read facial expressions. *J. Abn. & Soc. Psychol.,* 1929, **24**, 191-202.
7. Langfeld, H. S. The judgment of emotions from facial expressions. *J. Abn. Psychol.,* 1918, **13**, 172-184.
8. Ruckmick, C. A. A preliminary study of the emotions. *Psychol. Monog.,* 1921, **30**, No. 136, 30-35.

University of Nebraska
Lincoln, Nebraska

DOES POST-HYPNOTIC AMNESIA APPLY TO PRACTICE EFFECTS?[1]

E. F. Patten

Do practice effects acquired in the hynotic trance state carry over into the subsequent non-trance states even when the subject has a complete amnesia for the fact that practice has taken place? If the principle of post-hypnotic amnesia applies to practice effects as it does to ordinary recall, we should expect that the degree of performance efficiency in a normal state following a period of hypnotic practice should not differ from that preceding the period of hypnotic practice. If, however, the principle does not apply to practice effects, then the performance efficiency in the normal state following a period of hypnotic practice should be facilitated.

To obtain data for the construction of the requisite learning curves, it was necessary to have tasks for which college students possess relatively low initial ability, but in which practice gives rather rapid improvement. A constant increment addition test as described by Hull (1) was chosen as fulfilling these requirements. Hull says,

> "It is so completely 'mental' that it does not require even the writing down of answers by the subject. No apparatus is

---

[1] The writer wishes to acknowledge his indebtedness to Professor Clark L. Hull for his aid in the pursuance of this study.

> required. The subject takes up any comfortable position that he prefers. The experimenter says 'Ready,' starts his stopwatch, and at the same time calls out a two-place number such as 26. The subject then proceeds to add to this the digit 6, to that total 7, to that total 8, then 6, then 7, then 8, and so on continuously as rapidly and accurately as possible, calling out the totals (only) as they are obtained. Thus the first few totals starting as indicated above would be: (26) 32, 39, 47, 53, 60, 68, 74, and so on. When 100 is reached, the hundreds are dropped from the totals, the subject never speaking more than the last 2 digits. At the end of 30 seconds the experimenter interrupts the subject by speaking a new number—say, 84. The subject immediately begins adding to this number the 6, 7, and 8 as before, and continues for 30 seconds, when a new number is given, and so on. The adding is continued for 5 minutes at each test and yields 10 sets of additions. For recording the performance in this test, special mimeographed sheets were prepared containing in parallel columns the totals for the various starting numbers as far as 25 additions each. The experimenter would call out the number at the top of the column and then follow along down the series of totals with his pencil as the subject spoke them, until 30 seconds had elapsed, when he would draw a line beneath the last total given, at the same time calling out the number at the top of the next column. In case the subject made an error part way down the column, as often happened, the experimenter recorded the incorrect total on the sheet by the side of the corresponding correct one, and beneath this in a column all the following totals given during the remainder of the half minute. This was necessary because, once an error is made, all the remaining totals will differ from the printed series even though all the subsequent additions be correct. Occasionally very rapid adders succeeded in making more than the 25 additions in the allotted 30 seconds, in which case the additional totals given were recorded beneath the appropriate column" (1, p. 104).

The only exception we made to this procedure was the inclusion of the digit 9 in the series, making 6, 7, 8, and 9 to be added in order. The score for each practice period was the total number of correct additions performed in the 5 minutes. The error score was the total number of errors made during each five-minute period.

Fourteen subjects were used, all college students who volunteered for the experiment. Half of these subjects were those in whom we could induce what we considered deep hypnosis.

TABLE 1

Showing the Number of Correct Additions Performed by Each of the Fourteen Subjects on Each of the Eighteen Days of Experimentation

On days from 7 to 12 inclusive the adding by the hypnotic group was performed in the trance state.

Subject	Yr.	Sex	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
									Hypnotic Subjects											
A.L.	2	M	60	109	99	119	119	117	113	118	120	123	123	136	127	169	163	169	166	170
R.S.	3	M	47	71	82	84	61	87	69	90	82	91	109	82	94	122	101	109	95	112
A.P.	2	M	75	94	91	97	103	98	131	145	141	141	125	182	152	162	129	117	170	182
M.S.	1	F	56	47	58	66	53	68	70	62	69	76	83	84	99	97	108	109	113	123
H.C.	1	F	74	107	115	126	138	135	142	145	168	163	163	179	167	168	197	189	194	205
M.C.	2	F	83	91	97	102	110	113	95	115	114	129	137	135	158	156	158	147	178	179
M.R.	2	F	72	91	110	103	118	98	117	139	143	144	142	152	130	146	159	169	182	161
Mean			66.72	87.15	93.14	99.57	100.29	102.29	105.29	116.29	119.57	123.86	126.00	135.72	132.43	145.72	145.00	144.15	156.86	161.72
									Control Subjects											
R.M.	2	M	143	160	196	207	223	241	269	248	275	285	297	305	320	296	332	319	352	376
H.M.	3	M	138	133	146	156	161	163	180	192	196	171	175	181	153	179	178	180	204	207
C.B.	2	M	75	83	91	97	101	105	108	104	116	108	106	114	113	107	113	109	118	128
E.S.	1	F	101	115	124	119	125	126	140	146	154	139	158	171	167	177	181	190	197	215
M.S.	1	F	85	109	114	109	106	117	112	121	129	111	119	136	128	118	128	123	116	126
M.W.	2	F	114	131	151	139	158	174	160	168	147	151	163	205	181	203	207	165	209	215
G.P.	2	F	73	103	91	111	133	117	123	129	136	160	155	148	186	168	188	206	207	173
Mean			104.14	119.14	130.43	134.00	143.86	149.00	156.00	158.29	164.72	160.72	167.57	180.00	178.29	178.29	189.57	184.57	200.43	205.72

The other seven were control subjects, each being paired off against one of the hypnotic group as to sex and scholastic attainment. These control subjects added in the manner described for 18 successive days.

Each member of the hypnotic group added thus for the first six days, but at the beginning of the seventh practice period was deeply hypnotized and made to perform the additions while in the hypnotic state. That the subjects had amnesia for the work done in that state is shown by the almost invariable question upon dismissal after waking at the end of the practice period, "Aren't you going to have me add today?" The practice for the eighth, ninth, tenth, eleventh, and twelfth days was likewise performed during hypnosis. From the thirteenth to the eighteenth days inclusive these hypnotic subjects performed their additions during waking or normal states as in the first six days.

We have, then, comparative records from:

> 1. Seven hypnotic subjects, who added while in the normal waking state for six days, then while under hypnosis for the next six, and, finally, while in the normal state for the last six of the eighteen days.
> 2. Seven control subjects who added in the normal state for eighteen successive days.

The number of correct additions made by each of the subjects for each of the eighteen practice periods is given in Table 1. The average daily performance of each group is also given.

In Figure 1 the averages are represented graphically. Here the values along the abscissa represent the successive practice periods for the eighteen days. The height of the curves represents the average number of correct responses for each practice period. The solid line represents the daily averages for the hypnotic subjects, while the broken line shows the daily averages for the controls. Breaks appear after the sixth and again after the twelfth days to differentiate more clearly the various thirds of the total practice periods.

Since the two groups evidence a decided difference in ability, it is somewhat difficult to compare the shape of the curves directly in Figure 1. To facilitate this comparison, one curve is superimposed upon the other in Figure 2. Here the curves are started on the same point, the subsequent ordinate values being the differences between these initial scores and the average scores on the successive days.

The curves in Figure 1 are approximately of the same shape; those of Figure 2 practically coincide. It is plain that the last six-day period of non-hypnotic addition of the hypnotic subjects is influenced by the preceding period of practice for which these subjects have amnesia, quite as much as was the case with the non-hypnotic group. We may thus conclude that post-hypnotic amnesia does *not* apply to practice effects.

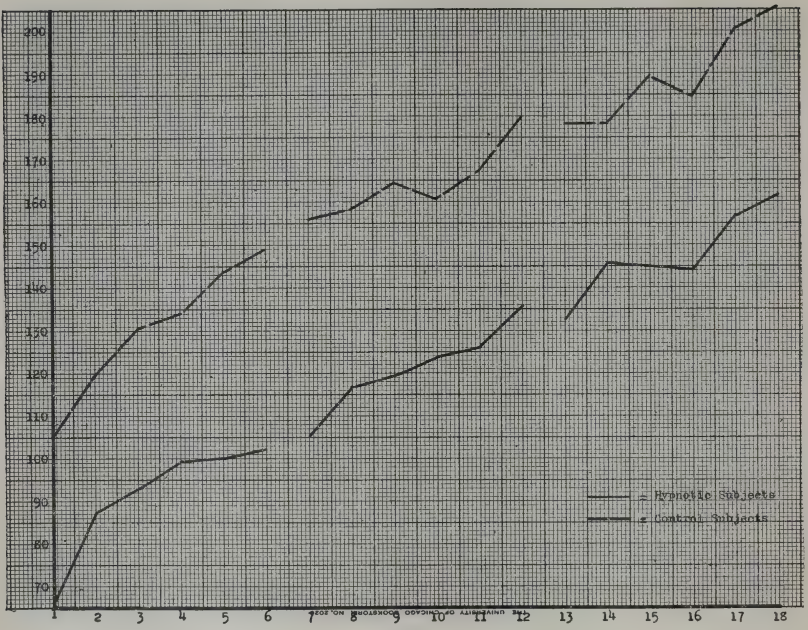


FIGURE 1
Number of Correct Additions

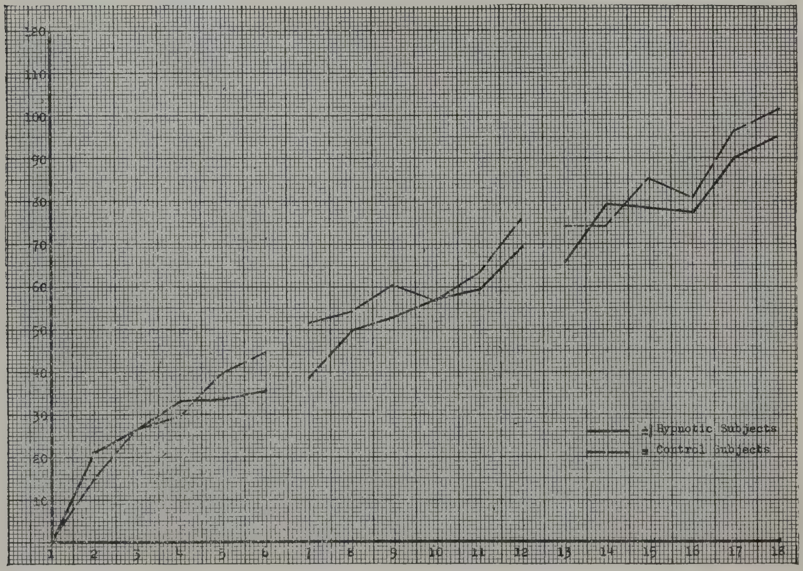


FIGURE 2
Difference between Number of Correct Additions on First Day and on Subsequent Days

An inspection of Figures 1 and 2 also shows that both the speed of adding and the rate of increase in speed as the result of practice while in the trance state are in no way distinguishable from the corresponding behavior in the normal state as manifested by the control group. No explanation of the greater facility in adding of the control subjects as a group is offered, save that it may be a sampling deviation of unusual magnitude.

Four of the subjects of each group were women and three were men. Plotting the averages according to sex seems to reveal no significant differences, so these curves are not included in this report.

Although a careful record was kept of the errors in additions made by each subject each day, there seem to be no unmistakable tendencies revealed when these error averages are plotted. As a result they also are omitted.

REFERENCE

1. HULL, C. L. The influence of tobacco smoking on mental and motor efficiency. *Psychol. Monog.*, 1924, **33**, No. 150. Pp. 160.

Miami University
Oxford, Ohio

EMOTION, INTELLIGENCE, AND INTROVERSION

EDWIN G. FLEMMING

The purpose of this investigation is to study the relation between intelligence and emotion, between introversion and emotion, and the inter-relations among the intensity, the variability, the deviation, and the consistency of emotional responses.

TESTS USED

For the measure of intelligence the Thorndike Examination for High School Graduates was used.

The Colgate Schedule C2, Laird's test of introversion-extroversion was used as the measure of introversion.

For a measure of emotional responses the author revised the Pressey X-O test for investigating the emotions. Parts I, III, and IV of the Pressey test were used; Part II is an association test and could not be arranged for use in the revision. The words of the Pressey test were arranged in a column instead of in rows, and opposite each word were the figures from minus five, through zero, to plus five.

The directions for Part A of the revised test were as follows:

> Opposite each of the following words draw a circle around the number that indicates the degree of pleasant or unpleasant

> meaning that the word has for you. If the word has a decidedly unpleasant meaning, draw a circle around the minus 5 at the extreme left; if it has a decidedly pleasant meaning, draw a circle around the 5 at the extreme right; if it is neither pleasant nor unpleasant in meaning to you, draw a circle around the 0. If it is only relatively pleasant or unpleasant, draw a circle around the number which most adequately represents the relative degree of pleasantness or unpleasantness. The minus sign signifies the unpleasant side of the scale; no sign represents the pleasant side of the scale.
>
> *Do not sudy over your responses; immediate judgments are desired. Encircle only ONE number for each word, but be sure to indicate your response to EVERY word.*

Similar directions were given for Part B and Part C, except that in Part B the words represented something praiseworthy or blameworthy, and in Part C they represented things worrisome, fearful, or dreadful, on the one hand, or joyful, on the other.

Four different scores were calculated on this test and used in this study.

1. The intensity of emotional response—the sum of all the figures encircled regardless of sign. The reliability coefficient of this score is .74.

2. Variability of intensity—the sigma of the distribution of intensities regardless of sign. The reliability coefficient of this score is .70.

3. Deviation—first the average rating for each word by the entire group was determined. Since the group rating showed different variability on the various words, the *P.E.* of the group average for each word was figured, and the deviation score based upon the average and the *P.E.* Ratings of any subject within one *P.E.* of the average of the group were not considered deviations; but scores of deviation were given for each *P.E.* of deviation approximately on either side of the average. The deviation score then represents the total *P.E.* deviation regardless of the direction of the deviation. The reliability coefficient is .69.

4. Emotional consistency—This score was obtained by repeating the test and counting up the total amount of change that the subject made in his ratings. The reliability coefficient of this score is not known.

THE SUBJECTS

The subjects were 89 college men at the College of William and Mary, mostly of sophomore rank, with a sprinkling of juniors and seniors. There were no freshmen. However, there were scores on the Thorndike test for only 62 of them, while on the test of introversion there were scores for 74. The correlation between emotional consistency and Thorndike is based upon 53 cases, and the correlation between introversion and emotional consistency upon 67 cases. The correlations among the various measures of emotion are based upon the full 89 cases, except that correlations with emotional consistency involve 78 cases.

RESULTS

The correlations between the Thorndike score and the various measures of emotion on each part of the test and for the entire test are indicated in Table 1.

TABLE 1

SHOWING THE CORRELATIONS BETWEEN THE THORNDIKE SCORE AND THE MEASURES OF EMOTIONAL RESPONSE INDICATED

	Part A	Part B	Part C	A, B, and C
Intensity	—.51	—.36	—.27	—.43
Variability	—.13	—.02	—.26	—.16
Deviation	—.31	—.46	—.42	—.51
Consistency	—.32	—.26	—.24	—.35

From this table it appears that intelligence as measured by the Thorndike test is negatively associated with intensity of emotional responses, with deviation of the subject from the group norm, and with the consistency of the subject's emotional responses. The tendency is for the more intelligent to be less intense, to deviate less, and to be more consistent.

The correlations between introversion and the various measures of emotion are indicated in Table 2. It appears that introversion as measured by the Laird test is not definitely associated with any of the measures of emotion, except that there is a bare tendency for the introvert to be less consistent in his fear responses than the extrovert, as indicated by the correlation of .22 on Part C.

TABLE 2

SHOWING THE CORRELATIONS BETWEEN INTROVERSION AND THE MEASURES OF EMOTIONAL RESPONSE INDICATED

	Part A	Part B	Part C	A, B, and C
Intensity	—.05	—.02	.12	.01
Variability	—.13	—.05	.17	.02
Deviation	.06	.17	.16	.16
Consistency	.03	.01	.22	.12

Now let us turn to the measure of emotional consistency, the total extent of change of response upon retest. This objective test is in terms of emotion itself; and so if it relates to any of the other measures of emotion we may be able to induce some laws of emotion.

Table 3 shows the correlations between emotional consistency, on the one hand, and intensity, variability of intensity, and the total extent of the deviation in either direction from the group norm, on the other hand.

TABLE 3
SHOWING THE CORRELATIONS BETWEEN EMOTIONAL CONSISTENCY AND THE MEASURES INDICATED

	Part A	Part B	Part C
Intensity	.48	.41	.48
Variability	.35	.13	.47
Deviation	.67	.49	.60
Multiple correlations with consistency			
Deviation plus variability	.78	.52	.66

From this table of correlations we can state three laws of emotional response.

1. The more intense an individual's responses are to a series of stimuli, the greater is the tendency to change the responses to the same stimuli at a later time.

2. The more variable the intensity of a series of emotional responses is, the greater is the tendency to change the responses to the same stimuli at a later time.

3. The more an individual deviates from the group norm in the intensity of his emotional responses to a series of stimuli, the more likely is he to change his responses to the same stimuli at a subsequent period.

From the multiple correlation we are justified in stating that from a knowledge of the amount of variability in the intensity of emotional responses and of the amount of the deviation from the group norm, we can predict with a fair degree of certainty the amount of inconsistency or the total amount of change in an individual's emotional responses to a series of stimuli when confronted with the series a second time.

The correlations between variability and intensity are on Part A, .11; on Part B, .26; and on Part C, .30. Since the association is so slight, we may state only tentatively a fourth law of emotional response to the effect that an individual who is very intense in his emotional responses is also likely to show a tendency toward greater variability in his responses.

The correlations between intensity and deviation are on Part A, .55; on Part B, .33; and on Part C, .69; from which we may state a fifth law of emotional response: The more intense an individual's responses are the more likely is he to deviate from the normal response of the group.

From the correlations between variability and deviation no positive law is statable, since the correlations are on Part A, —.07; on Part B, —.04; and on Part C, .33.

CONCLUSIONS

From this study we may state several tendencies with respect to intelligence and emotion:

1. The more intelligent individual tends to be less intense in his emotional responses.

2. The more intelligent individual tends to deviate less in his emotional responses from the norm of the group.

3. The more intelligent individual tends to change his emotional responses to a lesser extent when confronted with the stimuli a second time.

There appears to be no definite relation between introversion and intensity of emotional responses, variability, or deviation from the group norm. There does, however, appear to be a bare tendency, probably of not much significance, for the introvert to change his fear responses when confronted with the stimuli a second time.

In addition, several laws of emotional response may be stated.

1. The more intense an individual's emotional responses are to a series of stimuli, the greater is the tendency to change the responses to the same stimuli at a later period.

2. The more variable the intensity of a series of emotional responses is, the greater is the tendency to change the responses to the same stimuli at a later time.

3. The more an individual deviates from the group norm in the intensity of his emotional responses to a series of stimuli, the more likely is he to change his responses to the same stimuli at a subsequent period.

4. (Tentative.) The more intense an individual is in his emotional responses generally, the more likely he is to be variable in his emotional responses.

5. The more intense an individual is in his emotional responses, the more likely is he to deviate from the normal intensity of the emotional responses of the group to which he belongs.

29 Claremont Avenue
New York City

CONSTANT ERRORS IN THE ATTEMPTED MEASUREMENT OF INTROVERSION

Joseph V. Hanna

There is much difference of opinion among psychologists at the present time as to the exact nature of those personal qualities which go to make up so-called introversion. These differences are not so apparent with respect to the general nature of introversion as originally described by Jung, as they are with respect to the various devices which have been evolved for the attempted measurement of this group of involved, subjective factors. It seems unnecessary to point out here that results from many of such scales or questionnaires in actual use correlate not very highly with one another. This is due, in large measure, no doubt, to the failure of individuals making the tests to agree upon any group of subjective responses as being commonly accepted criteria of introversion.

Inquiry into the reliability of the measure of any subjective trait such as that for introversion reveals difficulties quite as significant as those characteristic of the inability of different specialists to arrive at a common interpretation of the quality on the basis of objective criteria. The characteristic difficulties, apart from such as are experienced in standardizing more objective tests, arise out of the ever-changing nature of the subjective quality in comparison with those more permanently fixed, and out of the possibility that one subjective response to an outside stimulus (in this case the question, free-association stimulus, etc.) will not be identically the same as a subsequent response. A low correlation between two series of subjective test scores separated by an interval of time for the same group of students might conceivably be due either to personality changes within the individuals, or to low reliability of the tests. While the data here reported involve the factor of reliability, certain constant errors assume even greater significance than the degree of reliability.

In the first semester of the school year 1928-1929, a battery of psychological and personality tests was given to several hundred students enrolled in seventeen introductory psychology classes at Washington Square College, New York University. These students were college sophomores, with a sprinkling of juniors and seniors. The tests were administered in connection with a general survey, the purpose of which need not here be gone into. Included in the test battery was the Colgate Personal Inventory C2 for Introversion-Extroversion. It is in connection with this Inventory that the present data are submitted.

Through cooperation with other members of the psychology staff, the writer administered to 53 students who had taken the battery of tests in 1928-1929, the Colgate C2 Inventory, just before the close of the first semester of the academic year, 1929-1930, or one year after the original testing. The coefficient of correlation between the two rankings of the 53 students, separated by the interval of a year, is .59 ± .06. It may be added that the Colgate C2 Inventory correlated approximately zero with scholarship, and with intelligence as measured by the Otis Advanced Group Intelligence Test and Part III of the Thorndike Examination for High School Graduates, administered to the several hundred students previously referred to.

The rather low coefficient of correlation may be attributed to two possible explanations. Either the qualities aimed at in the attempted measurement had changed within the year, or the Inventory itself, due to its content, arrangement, or method of administration, is not highly reliable. This question would seem to be partially answered by the presence of trends which appear when a closer examination of the respective scores for the separate testings is made.

Scores for the 53 subjects for the two testings are presented in Table 1. In the first column of the table appears the number of the subject (based on approximate rank in the first testing), in the second column the score the subject made in 1928-1929, and in the third column the score made in 1929-1930. In the "difference in score" column appear the differences between the 1928-1929, and the 1929-1930 scores. If the subject's 1928-1929 score is larger than his 1929-1930 score, the margin of difference appears in the "minus" side of the column and, if smaller, in the "plus" side of the column.

Examination of the scores appearing in Table 1 shows that 26 of the subjects made higher scores and 23 subjects made lower scores in 1929-1930 than in 1928-1929, 4 subjects making identical scores in the separate testings. Further examination reveals that the majority of those making higher scores on the second testing are in the lower (less introvert) half of the distribution, while the majority of those making lower scores are in the upper (more introvert) half of the distribution, on the basis of the first testing. Of the 26 subjects in the upper (more introvert) half, 15 made lower scores, 10 made higher scores, and one made an identical score on the second testing, whereas of the 26 in the lower (less introvert) half, 16 made higher scores, 8 made lower scores, and 2 made identical scores, on the second testing. In other words, the more introvert subjects tended, as a group, to score as less introvert, and the more extrovert tended to score as less extrovert upon being retested after the interval of a year.

The tendency of the more introvert subjects to make less introvert scores on the retesting is indicated not only by the numbers of introverts making less introvert scores on the retest, but also, and more significantly, by the actual extent of score deviations. Excepting for the near-absence of upward changes by subjects in the upper third (the more introvert subjects) on the retest, the size of score deviations is not so significant as an index of the trend toward less extreme extroversion. Whereas the larger percentage of upward changes appear in the lower two-thirds, and particularly in the lower half (less introvert and extrovert subjects) of the distribution, the size of deviations toward greater extroversion are about the same throughout the lower two-thirds of the scale range. Medians for the 1928-1929, and the 1929-1930 scores are 16 and 16.5, respectively, showing approximately the same central tendency for the two groups of scores.

On the basis of data already reported there seemed to be no certain method of determining the extent to which the tendency toward less extreme introvert and extrovert scores (a general shifting from extremes toward the center of the distribution) could be explained on the basis of chance variation, of some factor inherent in the blank which makes for constant error, or to possible personality changes of the subjects. It was decided to use a second group of subjects who could be retested at the end of such

TABLE 1

Two Groups of Introversion-Extroversion Scores for 53 Subjects

Approximate rank of subject (first testing)	1928-1929 score	1929-1930 score	Difference in score	
			“Plus”	“Minus”
1	31	28		3
2	29	25		4
3	29	20		9
4	28	23		5
5	26	16		10
6	26	15		11
7	25	22		3
8	25	21		4
9	24	28	4	
10	24	17		7
11	23	12		11
12	22	25	3	
13	21	13		8
14	21	21		
15	20	26	6	
16	19	15		4
17	18	4		14
18	18	21	3	
19	18	17		1
20	18	23	5	
21	17	32	15	
22	17	19	2	
23	17	13		4
24	16	28	12	
25	16	21	5	
26	16	19	3	
27	16	16		
28	15	23	8	
29	15	18	3	
30	15	17	2	
31	15	14		1
32	15	14		1
33	14	20	6	
34	14	20	6	
35	14	14		
36	14	8		6
37	14	16	2	
38	13	22	9	
39	13	21	8	
40	13	15	2	
41	13	12		1
42	13	10		3
43	12	16	4	
44	12	13	1	
45	12	9		3
46	11	16	5	
47	11	10		1
48	9	12	3	
49	7	6		1
50	6	11	5	
51	5	12	7	
52	5	5		
53	2	12	7	

TABLE 2
Two Groups of Introversion-Extroversion Scores for 36 Subjects

Approximate rank of subject (first testing)	First score*	Second score	Difference in rank	
			"Plus"	"Minus"
1	26	26		
2	26	18		8
3	24	25	1	
4	24	21		3
5	24	19		5
6	23	23		
7	23	18		5
8	23	17		6
9	22	23	1	
10	22	23	1	
11	21	18		3
12	21	14		7
13	21	17		4
14	21	17		4
15	21	15		6
16	20	21	1	
17	20	23	3	
18	19	19		
19	19	17		2
20	19	16		3
21	19	20	1	
22	19	16		3
23	19	17		2
24	18	17		1
25	17	15		2
26	16	16		
27	16	17	1	
28	15	11		4
29	15	19	4	
30	15	12		3
31	15	9		6
32	14	10		4
33	14	13		1
34	13	16	3	
35	13	11		2
36	13	9		4

*First and second testings were separated by an interval of three weeks.

shorter interval of time within which it would be impossible for any significant personality changes to take place. The writer selected a class of introductory psychology students for this purpose. The C2 Inventory was administered to this group (36 students), and was repeated three weeks later. There is a coefficient of reliability of .75±.065 between the two rankings of the 36 subjects. The median score for the first testing is 19, and for the second it is 18, showing a considerably more introvert group than the 53 subjects originally tested. The selection of the second group was unfortunate in this respect, but in spite of this difference the group serves the purpose for which it was selected reasonably well.

Scores for the 36 subjects constituting the second group are presented in Table 2. The arrangement of Table 2, being similar to that of Table 1, requires no further explanation. Twenty-three of the 36 subjects made less introvert scores, 9 made more introvert scores, and 4 made identical scores on the second as compared with the first testing. The tendency to make lower scores on retesting seems constant, and typical of scores falling throughout the entire scale range, in which respect the second group of 36 differed from the original group of 53 subjects. The 23 subjects of the second group made less introvert scores by a considerably wider margin than the margin characteristic of the 9 subjects who made more extrovert scores. The average increase in score for the 9 subjects making higher scores on the second testing is 1.8, as compared with an average decrease in score of 3.8 for the 23 subjects who show less introvert scores on the second testing. The tendency to change in score within the second (the more introvert) group would seem roughly comparable to that affecting above-median subjects in the original group of 53 subjects (the less introvert group). However, there seems to be no constant tendency for those making lower (less introvert) scores to make higher scores in the retest, as in the case of the testings separated by a year. Individual deviations are larger among scores for the two testings separated by an interval of a year than among scores separated by an interval of three weeks. Perhaps one of the most interesting findings of the study is the presence, after an interval of a year, of the two tendencies—for those making more introvert scores to make smaller scores, and for those making more extrovert scores to make larger scores—whereas, after an interval of three weeks, only the one tendency is discovered—for those making more introvert scores to make smaller scores. This may be in part due to the fact that the second group is smaller, and is considerably more introvert than the first. However, it is noteworthy that at the close of an interval of three weeks, as well as of a year, the individual deviations of scores seem to be related to constant tendencies toward score changes.

Speculation as to the definite explanation of the constant tendencies which, independently of signficant personality changes, make for such deviation in scores as have been noted would be useless. Further inquiries, however, might well be directed toward an objective analysis and evaluation of the blank itself, certain parts of which are vague, to the attitudes and motivation of the subjects, the extent to which they regard introversion and extroversion as assets or liabilities, and the like. As the blank is now used, the clinical significance of scores obtained would seem to be somewhat doubtful, especially in the case of individuals who have had previous experience with the questionnaire.

New York University
Washington Square College
New York City

PHYSIOLOGICAL AND PSYCHOLOGICAL CAUSES OF OCULAR FATIGUE

J. I. Kurtz

The study of the causes of ocular fatigue is of great importance because of the fact that fatigue brings on certain physiological abnormalities which are forerunners to the development of pathology. A close study of the cases which complain of ocular discomfort have disclosed that fatigue is the most outstanding factor which causes discomfort.

Fatigue, from a physiological standpoint, may be due to the exhaustion of substances used for cell nourishment and the increased production of the waste products. These waste products are claimed to be carbon dioxide, lactic acid, and potassium phosphate acid. They poison or depress the muscle. If any two of these products are to be injected into a fresh muscle, it will behave from the very beginning as if it were fatigued. From a psychological standpoint, fatigue may be caused by abnormal mental states in general and poor emotional adjustments in particular. We do not have any reliable test to discover existence of fatigue or to measure its amount, hence our only means for gaining information about this subject is the subjective method.

There are a number of points from which fatigue might occur (2) so as to lower activity: (1) fatigue of the muscle fibers, (2) fatigue of the nerve fibers, (3) fatigue of the motor cells, (4) fatigue of the junction of the motor nerve with the muscle fiber at the motor end plate, (5) fatigue of the synapses between the nerve cells, (6) fatigue of the sense organs and different apparatus, (7) fatigue of the centers of voluntary control. The general symptoms of visual fatigue are a sensation of roughness, dryness, burning and smarting, or feeling of foreign bodies in the eyes. To many patients these symptoms are natural indications that their visual apparatus is out of order, and it is this that makes them go to the optometrist looking for relief.

To others, again, these symptoms merely indicate that the eyes need a rest and consequently they stop fatiguing work. Intra-ocular pain or headaches are definite symptoms of ocular fatigue. There is, it seems, a close association between the eye symptoms and the brain symptoms. This may be due to the fact that the brain is depending for its information from its advance guard—the eye. "The thinking machine—the brain—works with certain tools," states H. C. Wood. "It is clear that if these tools or instruments be dull or out of order, an enormous loss of power must occur in using them. The most important of these tools are the special senses. It is of the first importance to have the organs of the special senses in good order. This process of eye-strain and brain-strain may go on for years until at last the individual is arrested by the giving out of the brain or by the retinal irritations becoming so severe that vision is no longer endurable." Sensory fatigue is usually found mostly in the case of the eye; its symptoms are made up partly of sensations of soreness and partly of fatigue of the eye muscles. An actual loss of the functional power of the eye may result from continued work. In considering "brain fatigue" we must bear in mind that there are several sorts of fatigue to be considered. They are, according to Ladd and Woodsworth (3, p. 538): First, the metabolic fatigue similar to that which exists in the case of the muscle. The second type of fatigue may be designated as "transferred metabolic." This results from the carrying of fatigue products by the blood from the muscle in which it is produced to some other organ which has been resting. This form of brain fatigue is real and is evidenced by the drowsiness and inability to do mental work immediately after severe muscular exercise. The third type consists of feelings and sensations of fatigue. While this may not be a true form of fatigue, it has at the same time a genuine inhibiting influence acting to cause the cessation of work, especially of the particular work which has been carried on. When a small group of muscles is repeatedly innervated, pain may develop in the muscles and their tendons. The tendency under this condition is either to stop the work or to change it to some other form whereby the action of some other group of muscles will be called into play. The fourth type of fatigue is at times classed as sensory adaptation. In the case of the eye, fatigue was the explanation of Helmholtz for after-images. There are histological changes in the retina on exposure to dark or to light, which are certainly not similar to the forms of fatigue so far described, but constitute a positively adaptive phenomenon. There is a considerable difference in metabolic fatigue, which actually lowers the functioning power of an organ and the inhibitions which result from the action of certain stimuli to the organ. These inhibitions are in most cases the cause of mental fatigue. The symptoms of this last-mentioned type of fatigue are (*a*) loss of interest, (*b*) feeling of being bored, (*c*) desire to change off and do something else, and (*d*) inability to concentrate. Brain fatigue, according to this discussion, is a lowering of energy in the cerebral centers. Work done under this condition wastes the cerebral stores of energy. Jackson (1) claims that fatigue of the retina and visual centers is of more importance than all other forms connected with vision; on this point we can hardly agree with him, because from clinical experience and observation we find that ocular fatigue due to abnormal functioning of the ocular muscles is just as important and just as harmful. The fact that we cannot distinguish between the fatigue which belongs to the retina and that which belongs to the intra-cranial neurons or external muscles will substantiate our claim. Also, it is a fact that coordination of each of the six muscles of the eye with each other is of prime importance in the proper functioning of the visual system, and the lack of this coordination is largely responsible for the major part of ocular fatigue. The stimuli to the muscles of the two eyes must be coordinated if binocular vision is to prevail. This condition of properly balanced stimuli cannot prevail when the energy flow of one or more nerves is slowed up due to existence of some toxic condition. It is important to differentiate the different types of fatigue and try to localize the essential changes that give rise to fatigue; also, we should not overlook the extremely important part that efforts of coordination play in producing ocular fatigue; this, claims Jackson, should prove useful in giving us a better conception of a condition that passes easily from a physiologic to a pathologic condition. When we once appreciate the importance of visual fatigue and coordination fatigue and their possible effects on the human system in general and on the nervous system in particular, we begin to realize the importance of the adaptation of proper therapeutic measures not only for the relief of the existing abnormalities but for the removal of the fatigue causes as well.

METHOD OF TREATMENT

In order to treat fatigue conditions properly, it is important that we know the cause responsible for this condition. While it is difficult to determine the exact causes of ocular fatigue, they all fall into two main classes:

1. Physiological
2. Psychological

Our refraction, along with the subjective symptoms complained of by the patient, should help us materially in classifying the case. If it is of a physiological nature, then our procedure should be (*a*) to correct the refractive error, if any, (*b*) to treat the muscular abnormalities, (*c*) to establish proper nerve energy flow by applying the proper type of electric current, (*d*) correcting the position or posture at which the work is being done.

If the case is of a psychological nature, then we may have to correct any one of the following conditions: (*a*) to provide proper illumination, (*b*) to change the surroundings and environment; the paint, the decorative scheme, the extreme color contrast may all be contributing causes, (*c*) mental attitudes and emotional adjustments. These may have to be carried out either at work or at home.

M. Luckiesh (4) is quite correct when he states that "the seeing specialist must open his mind to physiology as well as psychology."

REFERENCES

1. JACKSON, E. Ocular fatigue. *Amer. J. Ophthal.,* 1921, **4**, 119-121.
2. KURTZ, J. I. The principles and practice of ocular physical therapy for optometrists. *Amer. J. Optometry,* 1930.
3. LADD, G. T., & WOODWORTH, R. S. Physiological psychology. New York: Scribner's, 1887. Pp. 538.
4. LUCKIESH, M., & MOSS, F. K. Seeing—a partnership of lighting and vision. Baltimore, Md.: Williams & Wilkins, 1931. Pp. 248.

Foshay Tower
Minneapolis, Minnesota

APPARATUS

THE BEHAVIOR RESEARCH PHOTOPOLYGRAPH[1]

CHESTER W. DARROW

The Behavior Research Photopolygraph provides a compact, portable system giving photographic records of the galvanic skin reflex, blood pressure, respiration, time, and signal marks on six-centimeter bromide paper by the shadow-graphic method commonly employed in work with the string galvanometer. It affords sensitive, continuous, standard unit resistance records of the galvanic skin reflex without any of the vagaries incidental to amplification. It permits simultaneous recording of other changes, the indicators for which may be interposed in the path of the beam of light. Shadow recording has the advantage that records of different changes may overlap considerably and still be legible.

In the form here presented the apparatus consists of two separable boxes or carrying cases which may be set face to face. In the left-hand box, as indicated by numbers in Figure 1, are contained: (1) an automobile headlight lamp as a source of illumination; (2) a toy transformer supplying current to the lamp; (3) an electric clock suitably geared to vanes which interrupt the beam of light at second intervals; (4) a compact form of the Behavior Research Resistance Box[2] with shunt and batteries contained; (5) a small pointer galvanometer with a vertically extended pointer suitably housed with plane glass on one side and a cylindrical lens or prism on the other.

In the right-hand box, as indicated by numbers in Figures 1 and 2, are contained: (6) a blood-pressure recorder; (7) a respiration recorder with removable pointer; (8) a blood-pressure dial manometer; (9) a horizontal slit upon which light passing through the prism in the galvanometer housing is concentrated;[3] (10) a motor drive suitably geared to rollers to carry the photographic strip; (11) a rheostat (not shown) for controlling motor speed; (12) a magazine taking a 175-foot roll of 6-cm. bromide paper; (13) suitable rollers to pass the paper across the slit; (14) a winding spool driven by a spring belt (spool not shown); and (15) a signal marker. Each box is provided with a cover and leather grip for convenience in transportation.

The galvanometer is of the sensitive type recommended for hydrogen ion determinations.[4] The removal of the celluloid window permits a light paper

---

[1] Studies from the Behavior Research Fund of the Institute for Juvenile Research, Chicago, Series B., No. 188. We are indebted to Miss Rosamond Howland of the Art Department of the Institute for the preparation of the figures.

[2] Described in (1).

[3] Spun glass fibers were fastened across this slit at 2-mm. intervals to provide horizontal coordinates on the records.

[4] Leeds Northrup—Portable pointer Type D galvanometer.

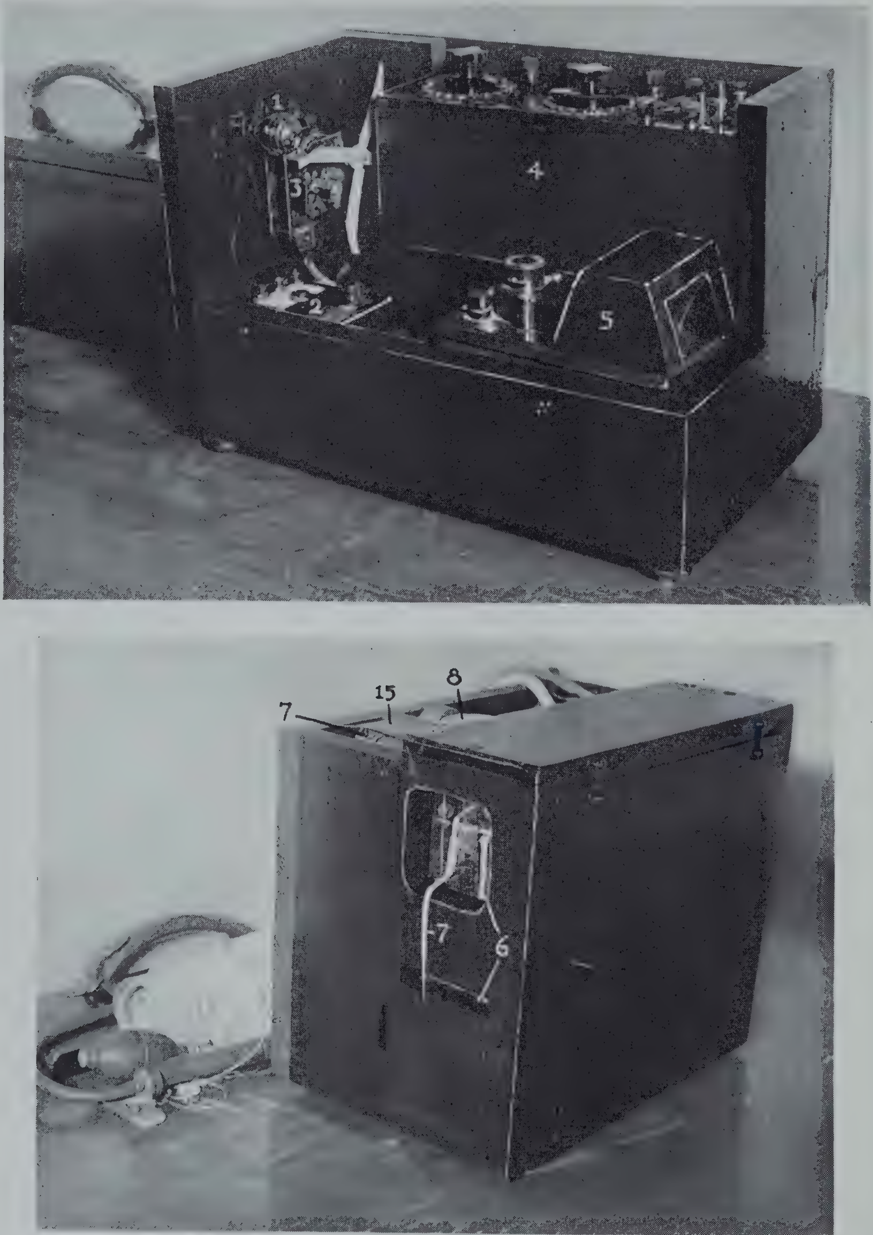


FIGURE 1
THE BEHAVIOR RESEARCH PHOTOPOLYGRAPH
(1) automobile lamp; (2) transformer; (3) electric clock marking seconds; (4) Behavior Research Resistance Box; (5) pointer galvanometer; (6) blood-pressure marker; (7) respiration marker and respiration bellows; (8) blood-pressure dial manometer; (15) string to signal marker. Covers with handles attached, pneumograph, blood-pressure cuff, and blood-pressure bulb are visible in the background.

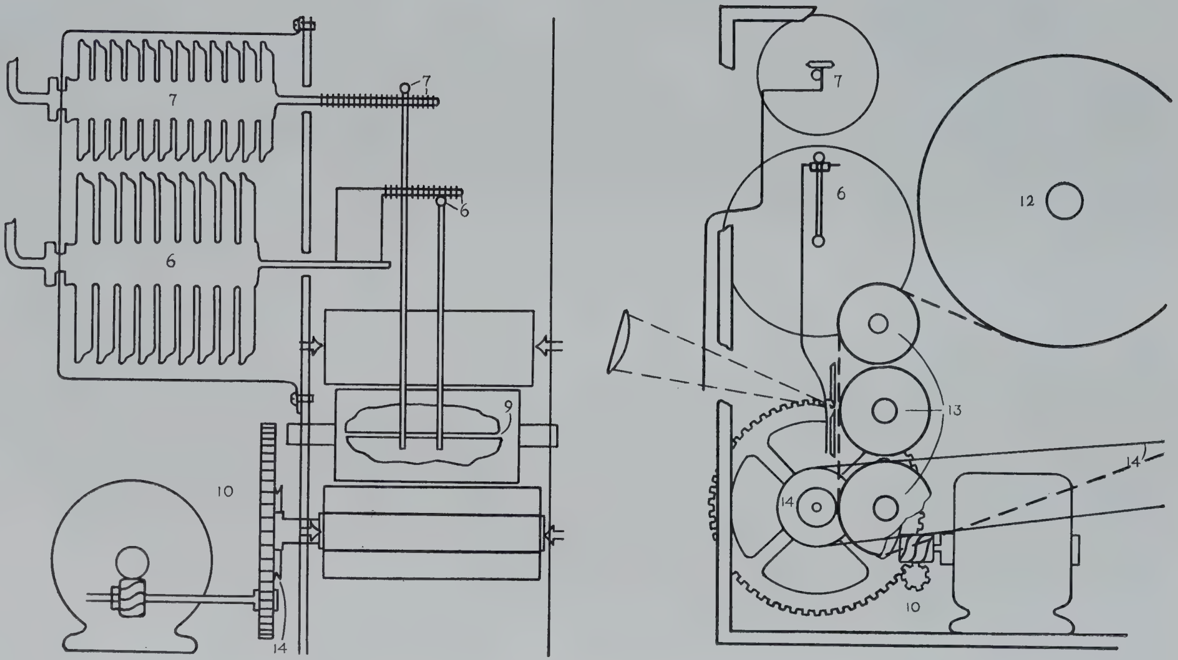


FIGURE 2

Diagrammatic Front and Side Views of the Recording Camera Mechanism

(6-6) Blood-pressure bellows and pointer; (7) respiration bellows and pointer; (9) exposure slit; (10) motor and gears; (12) paper magazine; (13) driving and guiding rollers; (14) pulley and belt to winding spool.

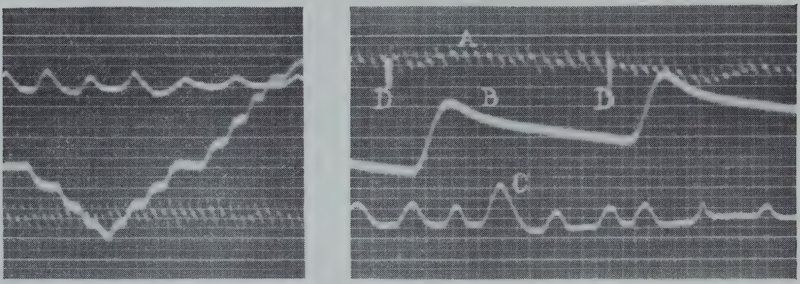


FIGURE 3

Sections of Record Made by Behavior Research Photopolygraph

The left-hand section shows galvanometer record of a standard scale of $1,000_{\omega}$ steps either way from the $100,000_{\omega}$ balance. The right-hand section shows a polygraphic record of (*A*) blood pressure, (*B*) resistance change, (*C*) respiration, and (*D*) signal marks. The vertical white lines are second marks.

indicator to be glued to the pointer without seriously disturbing the balance. The period of vibration of this instrument is fairly long, but this may be shortened considerably by increasing tension on the suspension wire, still leaving adequate sensitivity. To increase the tension an additional spring may be inserted beneath the spring supporting the upper suspension fiber. While the longer period of this galvanometer as compared with that of a string instrument is a disadvantage, it is adequate for all ordinary types of galvanic-reflex records. The recording of the standard-unit deflections at the beginning and end of a record is necessarily slower, about two seconds being required per step (see Figure 3).

The blood-pressure record is obtained with a 9-flange phosphor-bronze bellows 3 inches in diameter moving a rack which meshes with a pinion to which the pointer is attached, as shown at (6) in Figure 2. The respiration indicator uses a notched rod engaging a knife edge on the pointer. With such an arrangement, shifts in pressure within the respiration or blood-pressure systems which move the pointer out of the recording range may be compensated for by a light touch on the pointer, causing it to slip a notch into a new recording position. A signal marker is arranged to throw a shadow on the record when a string is pulled.

A sample record obtained with the apparatus is presented in Figure 3. The method of obtaining the standard scale for reading ohms deflection, and the method of reading the records are the same as described for the improved form of the Behavior Research Resistance Box (1).

Where it is desirable to have the records immediately available for inspection (as when using the machine as a "lie detector"), a developing apparatus may be substituted for the winding spool in the right-hand box.

Apparatus of this kind has been completed as this report goes to press. It consists essentially of two small rectangular glass trays fitting the bottom of the box, and rollers suitably arranged so that the photographic strip automatically passes first into developer, then into acid fixing bath, and then comes out of the box, ready for inspection.

Reference

1. Darrow, C. W. Uniform current for continuous standard unit resistance records. *J. Gen. Psychol.,* 1932, **6**, 471-474.

Behavior Research Fund
Institute for Juvenile Research
Chicago, Illinois

A METHOD OF MEASURING DIFFERENCE COLOR LIMEN BY MEANS OF THE PHOTO-ELECTRIC CELL WITH THERMIONIC AMPLIFICATION

Christian Paul Heinlein and Lucille E. Morgan

A quantitative study of the least difference in the intensity of two colored stimuli necessary to arouse a just noticeable difference for any given color or wave-length necessitates a consideration of the factors that inhere in the problem. First, there must be a means of presenting to the eye two homogeneous stimuli of the desired wave-length or color, and of keeping one constant while the other is varied by small degrees. Second, there must be a means of measuring the intensity of each stimulus. Third, the apparatus must be stable in its response from day to day. Fourth, such a necessarily sensitive device must include a simple means of recalibration for any adopted wave-length. Fifth, a device for measuring the light intensity of the stimuli must have a sensitivity exceeding that of the human eye. Sixth, the device must permit accurate reproduction of the experimental conditions. To devise such an apparatus which meets the requirements is the central purpose of this investigation.

The determination of difference limen for color requires the exposure of two homogeneous color lights of equal intensity, one of which is to be kept constant while the other is to be varied until the subject perceives a difference in the two colors.

The apparatus for presenting the stimuli is the Morgan Color Limen Box, which consists essentially of a small cabinet partitioned into two equal compartments, each separately illuminated, and each containing a circular aperture in which may be placed a color filter. The following description includes the construction details of the apparatus.

The box has a base of 8 by 10 inches and a depth of 8 inches. It is faced with a black, smooth-surfaced, hard rubber panel. Each compartment is lined with white poster board and is illuminated by eight 2.5-volt lamps arranged octagonally in sockets soldered 1¾ inches apart to a brass frame. The frame for each division is placed in an upright position directly back of the panel in such a way that the stimulus field is in the center of the bank of lamps.

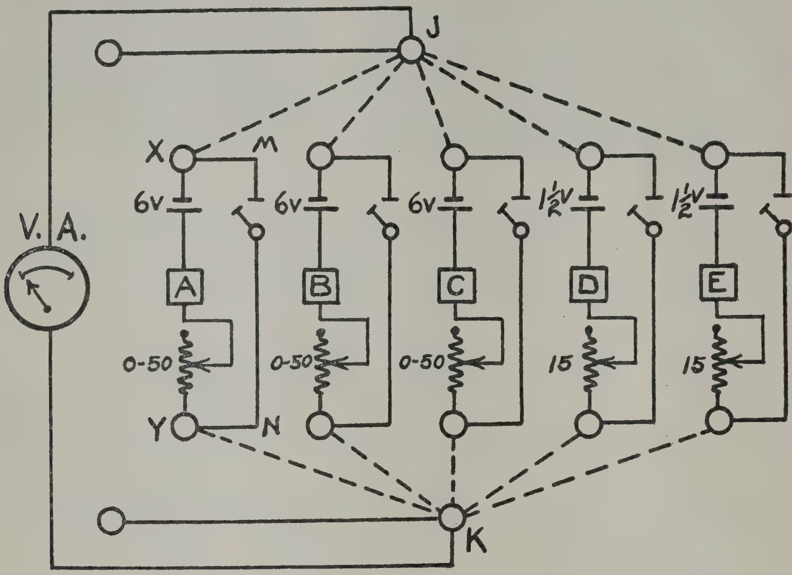


FIGURE 1
THE SWITCHING SYSTEMS

A and *B* represent the compartments containing the stimulus apertures. *C* represents a compartment which was not used in this study. It is directly back of compartment *B* and has an aperture in line with that of the front compartment. The purpose of the third compartment is to vary the illumination in compartment *B* by the addition of chromatic or achromatic light. It is found useful in studies requiring low intensities of illumination in the front compartment as well as in studies pertaining to color mixing. *D* represents the device used for dark adaptation, and *E* a small lamp, kept at a constant intensity, to illuminate the micro-ammeter. *XJKY* constitutes the measuring system, and *XMNY* the control system. The purpose of the first system is to make possible the measurement of the amperage draw of the illumination devices in each of the five circuits; the purpose of the second is to keep the lamps in the several circuits simultaneously illuminated.

For the control system five porcelain-base, single-pole, single-throw switches are mounted on the right end of the cabinet, the first in the circuit of compartment *A*, the second in the circuit of compartment *B*, and so on. (See also Figure 3.) All wires in the system are cut the same length in order to establish a series of equal resistances.

The measuring system consists of two switch levers, *J* and *K*, each having five contact points, located on the panel of the control cabinet. The lever poles are connected to an ammeter. By setting these levers on the appropriate pair of contact points, after opening the control switch, the lamp current is passed through the volt-ammeter (*V.A.*) and there measured.

As a means of varying the illumination in the compartments, a carbon disc vernier rheostat, rating 0-50 watts, is connected in the circuit of each bank of lamps. The two devices are conveniently mounted on a vulcanized rubber panel in the front of a radio cabinet. The current for the illumination of the eight lamps in each division is supplied by a 6-volt storage battery. The switching systems for the control of the lamp circuits are given diagrammatically in Figure 1, together with a description of their structure and function.

The heart of the apparatus lies in several devices which make possible the regulation and measurement of the intensity of each compartment in

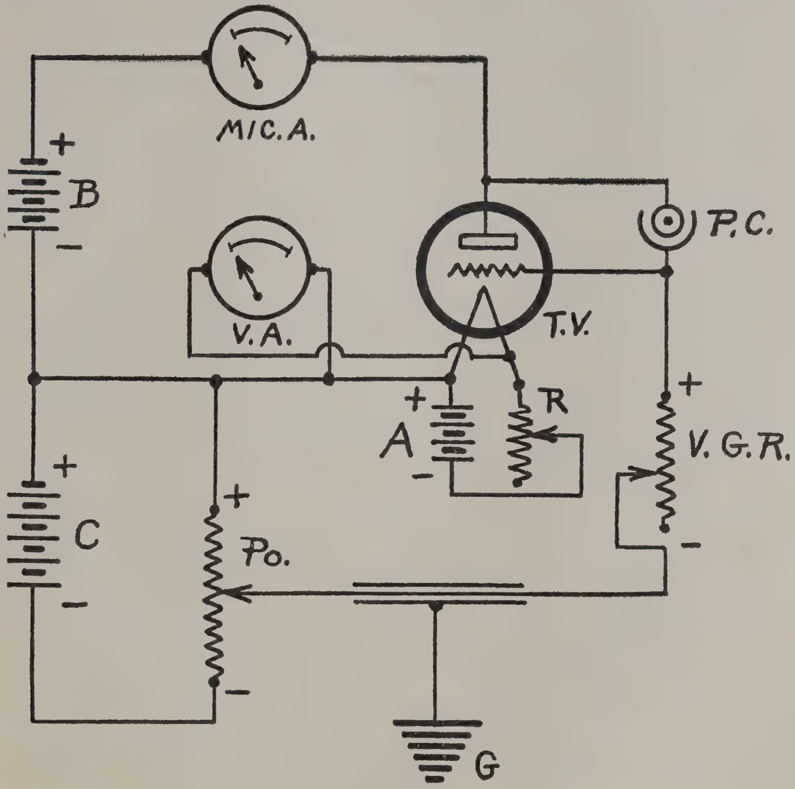


FIGURE 2

Circuit Connections of Photo-Electric Cell Amplifier

Mic.A., micro-ammeter; *V.A.*, volt-ammeter; *P.C.*, photo-electric cell; *R.*, filament resistance; *V.G.R.*, variable grid resistor; *Po.* potentiometer; *G.*, ground.

order to secure the basic conditions for the determination of difference limen for subjects. The two essential devices incorporated in the apparatus for measuring the intensity of the stimuli are the photo-electric cell, which measures directly the light intensity generated in each compartment, and a sensitive ammeter, which measures the current drawn by the eight lamps in each compartment.

The photo-electric cell employed in this study is of the caesium oxide type, and was selected chiefly because its greatest sensitivity is in the visible region of the spectrum. The cell was manufactured especially for this investigation by the Westinghouse Electric and Manufacturing Company.

Preliminary tests revealed the fact that the response of the cell to the very slight changes in the intensity of the two stimuli which were perceptible to the human eye was not sufficiently great to be measured directly with a sensitive micro-ammeter. Accordingly, in order to secure greater sensitivity, it was necessary to introduce a resistance-coupled thermionic amplifier. Westinghouse thermionic valve, type S593429, was chosen for this purpose because it operates with a high degree of reliability in photo-tube circuits.

The simple thermionic amplifier circuit was converted into a photo-electric cell amplifier circuit by connecting the anode of the photo-cell to the plate of the thermionic tube, the cathode to the grid, and by adding a variable grid resistor with a range of fifteen megohms. The complete circuit used is shown in Figure 2. In this circuit, the plate voltage plus the grid bias voltage acts as the potential for the photo-electric cell. By adjusting the potentiometer so that the grid of the amplifier tube is sufficiently negative with respect to the filament, the plate current is reduced to zero when the cell is dark. When the cell is illuminated, a current flows from the positive terminal of the B-battery successively through the meter, the cell, the grid resistor, the potentiometer, and the C-battery, and then back to the negative pole of the B-battery. This current produces a voltage drop across the grid resistor, and the flow is in such a direction as to cause the grid to become less negative with respect to the filament, the extent of this change to become more positive depending, of course, on the strength of the current. Due to this grid bias change, the current flowing through the thermionic valve is proportionately increased.

After being properly set up, the circuit requires only an adjustment of the grid bias. When this bias is so adjusted that the micro-ammeter reads zero when all light is excluded from the cell, illumination of the cell produces a positive change of grid potential and increases the plate current. Thus, the micro-ammeter, if once calibrated, gives a means of measuring the intensity of the light impinging on the cell.

The two stimuli introduced for difference limen discrimination may be made equal in intensity by adjusting the proper rheostats in the lamp circuits until the micro-ammeter reads the same when the cell is exposed to the light beams at equal distances from the reflecting sources. The intensity of a given color surface on either side of the limen box may be increased or decreased by respectively decreasing or increasing the resistance of the circuit. Such increase or decrease is introduced until the subject perceives a difference between the two stimuli which are kept qualitatively constant. The difference in the intensity of the two stimuli, perceived as different by the subject, may be determined from the change in the reading of the micro-ammeter.

In order to get valid results from a measuring device of this kind, a number of serious difficulties arising in the circuit had to be overcome. It was found that the most serious limitation to the extent of the amplification that can be obtained is intermittent current leakage in the base of the thermionic valve as well as in other parts of the circuit. This condition can be prevented by proper precautions. Such precautions are herein listed to serve as a guide to those who desire to make use of the photo-electric cell as a light intensity measuring device. In this investigation, all connections were soldered. It is vitally important to remove any trace of solder flux. All circuit connections were made with Number 18 B.S. gauge copper wire protected by grounded shields. The resistances themselves were selected with reference to quality and carrying capacity. The switches and resistances for control were mounted on the panel of the radio cabinet and properly shielded. The amplifier valve was conveniently located in one end of the cabinet. Due to the current leakage existing across the leads of the valve, the socket of the valve was discarded, and the filament leads were supported in the air by small glass insulators (see Figure 3). The variable grid resistor was also supported in the air by soldering a brass switch lever to the grid lead of the tube, with a binding post attached to support the grid resistor. The plate circuit was similarly mounted. The valve itself was shielded by means of a close-fitting metal cup which was grounded to a lead connecting a water pipe. Especially was it found essential to properly shield the grid circuit against any extraneous electrical or vibratory disturbances. It was found that the circuit could not be operated near alternating currents because of the electromagnetic fields generated. By way of illustration, electric fans could not be operated in the same room with the apparatus. To aid in preventing electrostatic effects, which greatly affect the stability of the circuit, the shielded wire connections were made as short as possible. The potentiometer employed was a 5000-ohm wire-wound device obtained from the General Radio Company. As a means of controlling the filament potential, a 7-ohm heavy-duty rheostat was connected in the filament circuit. Switches were inserted in all the battery circuits, of knife or toggle panel type, as convenience dictated. To insure that the micro-ammeter should never be overloaded, connections were made

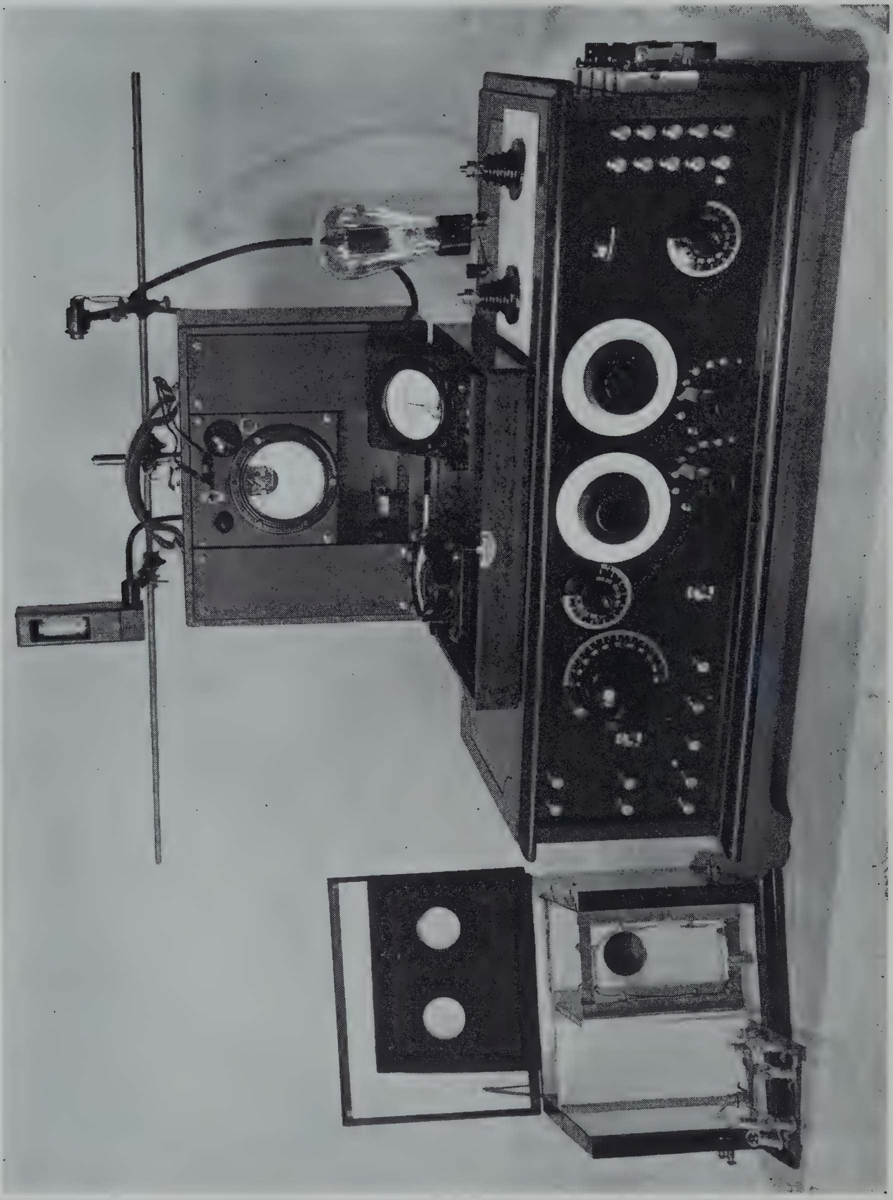


FIGURE 3

COLOR LIMEN DISCRIMINATION APPARATUS

The cabinet with raised lid at extreme left of photograph is the Morgan Color Limen Box. In order to show the character of the source of illumination, the rubber panel which supports the color filters has been removed and the bank of lamps in the left compartment lowered. To the right of the discrimination box is the cabinet enclosing the amplifier circuit and illumination control. The several dials mounted on the panel afford vernier control of filament and plate potentials, grid resistance, and lamp-bank current.

On top of the control cabinet may be seen the meters used to record the various circuit characteristics. Immediately above the micro-ammeter are mounted the photo-electric cell and standard calibration lamp. The thermionic valve has been removed from the control cabinet and placed on top for the purpose of illustrating the type of suspended mounting that was adopted to prevent any leakage effects.

such that by merely throwing a switch a preliminary test could be made with a less sensitive universal galvanometer.[1]

The amount of current drawn by each bank of lamps contained within each compartment of the limen box can be measured at any time through the switch control (described in Figure 1) connected in series with a sensitive ammeter.[2] While two adjacent compartments may each provide the same illumination value, the amperage readings may not necessarily be the same due to differences in the filament structure of the lamps and in the external circuit resistances. The usefulness of introducing an exact measure of the current consumption lies in the fact that a given ampere draw produces a given reading on the micro-ammeter when the cell is placed in the focal axis of the light beam at some specified distance. This system, therefore, serves as a check on the constancy of the photo-electric cell.

If the several parts of the apparatus produce the same results from time to time under the same measurable conditions, the requirements of the apparatus with respect to constancy are fulfilled. The constancy of the response of the measuring device is dependent upon the constancy of the filament and plate potentials of the thermionic valve in combination with the response of the photo-electric cell when exposed to light of a given intensity. If given filament and plate potentials repeatedly produce a given current output under constant illumination effects, the constancy of the entire system is proved.

Tests were made at frequent intervals over a period of five weeks. The filament of the thermionic valve was supplied with a constant potential of 5 volts, and the plate with 90 volts. The light source employed was a 3-candle-power standard lamp connected in series with a vernier rheostat for controlling the intensity of illumination. The photo-electric cell was placed one foot before the lamp. The response of the cell for various lamp-current-consumption-ratings was measured on the micro-ammeter. The results of such a test are given in Table 1.

A device that is sensitive enough to measure the smallest change in the intensity of colored light which the eye can detect must have a means provided for frequent recalibration in order to compensate for any slight alterations in the constants of the circuit which may result from long-con-

---

[1] The micro-ammeter used in this investigation was a Westinghouse instrument, Type DX, range 0-25 micro-amps. The galvanometer used was a Jewell Universal, Pattern 155. The sensitivity of the galvanometer was found to be .017 milliamps per scale division in calibration with a Weston milliammeter, range 0-1 milliamp, and a Leeds-Northrup Wheatstone Bridge. Since the galvanometer range covers 25 scale divisions, it is equal to .425 milliamps with an interposed resistance of 200 ohms. With the same interposed resistance, insertion of an eleven-ohm shunt across the leads increased the scale reading to 0-10 milliamps.

[2] The volt-ammeter was a Weston model 280. The instrument was tested with a Weston Set Analyzer (model 547) and found to calibrate accurately.

TABLE 1
A Representative Sample of Data Collected, Showing Constancy of the Apparatus

Dates of tests	Lamp current (amperes)	Response of photo-cell (micro-amperes)
June 10	.475	25.0
	.474	20.0
	.460	14.25
	.450	9.75
	.400	4.0
June 16	.475	25.0
	.474	20.0
	.460	14.25
	.450	9.75
	.400	4.0
June 24	.475	25.0
	.474	20.0
	.460	14.25
	.450	9.75
	.400	4.0
July 1	.475	25.0
	.474	20.0
	.460	14.0*
	.450	9.75
	.400	4.0
July 4	.475	25.0
	.474	20.0
	.460	14.25
	.450	9.75
	.400	4.0
July 11	.475	24.5*
	.474	20.0
	.460	14.25
	.450	9.75
	.400	4.0

*This table illustrates the type of response that may be expected from a succession of controlled stimuli when such stimuli are presented on occasions separated by longer intervals of time. The slight deviations in the readings on July 1 and 11 may be regarded as negligible when it is remembered that, with the introduction of a more sensitive ammeter to measure the current consumption of the stimulus lamps, the precision of the circuit is greatly increased.

tinued use. To secure a basis for translating the plate-current readings into actual intensity of illumination in foot-candle units, a one-point calibration using a one candle-power source at a distance of one foot from the photo-electric cell was performed, and the assumption entertained that for other intensities the response would be proportional to the degree of illumination. This assumption is justified on the basis of the manufacturer's research certification.

Two different one-candle-power sources were used in the calibration. First, a Westinghouse standard 3-candle-power, 8-volt incandescent lamp was operated by means of a series rheostat to serve as a secondary standard of one-candle-power as determined by careful tests made with a Leeds and Northrup Lummer-Brodhun photometer. The reference source in this case was a commercial standard lamp certified by the Nela Lamp Works. Second, largely for purposes of experimental interest, a standard sperm candle was used, and it is interesting to record that the two tests gave highly consistent results. The final result, as accepted, was a plate current of 20 micro-amperes for an illumination value of one foot-candle. The current flowing through the secondary standard required to produce one foot-candle was .474 amperes. In the absence of an exhaustive photometric investigation, it is impossible to state exactly what percentage of error the foregoing result may contain. The error to be expected in using a Lummer-Brodhun photometer ranges from 0.1 per cent to 1.0 per cent.

To facilitate recalibration at frequent intervals, the secondary lamp, together with controlling and measuring accessories, was permanently mounted on iron tripods, with a clamp to receive the photo-electric cell permanently fixed to insure automatically a distance of one foot between bulb centers. The procedure adopted for calibration consisted mainly in effecting a compromise between stability and sensitivity of the amplifier. Stability may be obtained by increasing the negative grid bias as controlled by the potentiometer. Increase in sensitivity may be gained by increasing the grid resistance as controlled by the vernier variable grid resistor. With the photo-electric cell darkened by means of a light-tight cover, the grid bias was adjusted until the micro-ammeter rested without fluctuation at 10 micro-amperes. The lack of any fluctuation at a non-zero reading indicated stability. The negative grid bias was then increased until the micro-ammeter read zero. Upon illuminating the cell, the desired sensitivity was obtained by increasing the grid resistance.

In order to measure retinal sensitivity, the apparatus must have a sensitivity greater than that of the retina. The test for this necessitates the introduction of human subjects. Problems consequently arise which are psychological rather than physical in nature, and provisions must be made for them in order to test the validity of the apparatus. Although there are a number of variable factors which influence the chromatic response of the retina, precautions were taken in regard to only three of the more outstanding: (1) state of adaptation of the retina; (2) purity of the stimuli; (3) and the size of the pupil.

A device was constructed for determining the degree of dark-adaptation.

A 2.5-volt lamp, fixed in the center of a metal cup mounted on a small block of wood, furnished the illumination for the device. A light-tight cover, sealed over the opening of the cup, contained a pinhole aperture in line with the filament of the lamp. To facilitate manipulation and intensity control, the device was connected into the switching systems shown in Figure 1.

Chromatic light for the stimulus fields was provided by one pair each of red, yellow, green, and blue filters supplied by the Corning Glass Works. The filters of a given color were of the same thickness and had the same transmission characteristics.

The size of the pupil for each subject was kept uniform by a pair of spectacles, the right half of which contained an artificial pupil one millimeter in diameter. The other half of the spectacles excluded light from the left eye.

Experimentation with the apparatus was conducted in a dark room. The box for presenting the stimuli was placed on a black table with black screens arranged on each side to prevent any admixture of stray light. The adaptation box was placed on the top of the illumination device. Upon entering the dark room, the subject seated himself in an ordinary classroom chair

TABLE 2

RESULTS OF SENSITIVITY TEST IN MICRO-AMPERES* AS MEASURED BY THE PHOTO-ELECTRIC CELL AMPLIFIER

(See text)

Subject	Reading	Red	Blue	Green	Yellow
A	1	4.5	2.5	.25	7.0
	2	4.5	2.5	.5	10.0
	3	4.0	2.0	.25	5.5
B	1	3.5	1.0	.25	4.0
	2	4.0	1.0	.25	3.0
	3	5.0	1.25	.25	5.0
C	1	20.0	3.0	.75	21.0
	2	21.0	2.5	.25	23.0
	3	17.5	2.25	.25	21.5
D	1	23.0	4.5	1.0	22.0
	2	20.0	3.5	.25	23.5
	3	25.0	3.0	.75	24.5
E	1	17.5	1.25	.75	21.0
	2	21.5	2.25	.50	17.0
	3	21.0	3.0	.25	17.0

*The apparatus was calibrated to measure one-foot-candle at 20 micro-amperes. No claim is made that the five subjects represent an adequate sampling of retinal sensitivity. The revelation of individual differences, however, is well illustrated. Provisions are made for increasing the sensitivity of the apparatus twenty-five fold. It is thus possible to obtain with the apparatus described a sensitivity of 500 micro-amps per foot-candle.

five feet from the adaptation box. He was instructed to fixate on the region directly in front of him, and, upon observing the small light, to press the button on the arm of the chair in which he was seated. After the signal was given, the spectacles were adjusted. His head was placed in a head-rest to insure a stationary position, the eyes being in direct line with the source of light. Each pair of the colored stimuli, each of the two equally illuminated, was presented to the subject. Upon seeing an increase in the intensity of the colored light in the right compartment, the subject was instructed to give the signal. Determinations were made in ascending series and measurements made directly with the photo-electric cell amplifier. The results for the subjects are given in Table 2. In reading this table, it must be remembered that the retinal sensitivity of Subject A to blue, for example, is not almost twice as great as that to red. In order that the limens for the various colors be strictly comparable, the results would need to be corrected in accordance with a sensitivity curve obtained for this individual photo-electric cell. Such a curve would indicate how the response of the cell per unit of energy varies with the wave-length. This phase does not come within the scope of the present investigation.

The final requirement in evaluating an apparatus for measuring difference color limen is that the device should permit reproduction of experimental conditions. The apparatus is made up of standard material and could be reproduced within certain limits. In attempting to construct a duplicate apparatus some differences of performances would in general be produced by variations known to exist among different photo-electric cells, even if of the same manufacture. The same is true, to some extent, of thermionic valves. For those parts of the apparatus which are not of standard types, the details of construction have been given.

Conclusions

The apparatus herein described serves as a means of measuring difference color limen. (1) A means has been provided for presenting two visual stimuli of the desired wave-length, and for keeping one constant while the other is varied by smaller degrees than can be detected by the human eye. (2) A means has been provided for measuring in arbitrary units of sufficient size the intensities of the two stimuli. The intensities thus obtained may readily be reduced to standard or universal units by correcting them in accordance with the color sensitivity curve of the photo-electric cell and the transmission characteristics of the filters used, as determined by spectrophotometric methods. (3) The apparatus has greater sensitivity than the human eye. (4) The response of the apparatus is stable from day to day. (5) A means has been provided for frequent recalibration. (6) Experimental conditions may be reproduced within the limits inherent in the parts of the apparatus themselves.

The methods used to gain the ends mentioned in these conclusions meet the central problems of measuring the difference limen of subjects. The apparatus is accordingly offered as a means of investigating this aspect of color vision.

Florida State College for Women
Tallahassee, Florida

BOOKS

EDITED BY
KARL BÜHLER
W. J. CROZIER
JAMES DREVER
A. MICHOTTE
JOHN PAUL NAFE
A. L. SCHNIERMANN
RAYMOND H. WHEELER

EDWARD S. ROBINSON. *Association Theory To-day.* New York: Century, 1932. Pp. viii+142.

The theory of association is a central feature of modern American psychology, but it has, nevertheless, received in recent times anomalous treatment. At the hands of the gestaltists, in particular, it has been set upon until one who reads only gestalt pronouncements must surely believe that the slings and arrows of the Whole have dispatched it irrevocably, weakened as it had supposedly been by the bacilli of atomism. But, in spite of this alleged demise, large numbers of psychologists have gone calmly on interpreting experimental results and systematizing generally as if no obsequies had been celebrated in Berlin and in Lawrence. Beyond a few scattered and general rebuttals, there has been no attempt to defend the theory, which is, in view of its ubiquitous and continued use, anomalous.

The attacks of gestaltists aside, moreover, the theory of association has stood for years in need of systematic re-editing. The modern version is clearly not that of the period of the British empiricists, nor is it even that of 1913, yet it has gone for years with no attempt having been made to give it the critical examination and contemporary systematizing which its importance and modern status merit.[1] The appearance, therefore, of a critical essay upon the subject is both welcome and important.

The associationism which Professor Robinson has formulated, while faithful to the historical perspective in which it finds itself, is no pale palimpsest compounded of historical doctrines. That this is true a brief survey of its tenets will presently show. Nor is it the anemic object which the critics see. It pervades contemporary American psychology to the point where it cannot be repudiated without at the same time rejecting a majority of systematizers and, one might add, a majority of experimentalists, since this same associationism is implicit in both the set-up and interpretation of a plurality of the published results upon problems to which association could in any way be construed as relevant. The doctrine is, therefore, characteristic and fundamental.

---

[1] Professor Carr's recent paper, "The laws of association," *Psychol. Rev.*, 1931, **38**, 212-228, is a notable exception to this current lack of a critical examination of the theory.

The association of which Professor Robinson writes means, in its general sense, *"the establishment of functional relations among psychological activities and states in the course of individual experience"* (p. 7). To many psychologists, "who stand in awe before the educated salivation of a Russian dog" (p. 7), the conditioned response usurps the center of the stage. To the author, however, the doctrine of the conditioned response is, in its theoretical form, scarcely more than an attempt to embroider the more general theory of association. The critical examination of the broader objections to the concept of psychological association, objections which are reviewed and found to be no more applicable to the concept than to most of the other widely accepted notions of science, finds the conditioned response doctrine more vulnerable than associationism proper.

The problem of the items which are associated has for some time wandered in the scientific half-world of issues lacking explicit formulation. A few writers have treated it, but the more common procedure has been, when the answer in terms of "ideas" has been avoided, to let association connect whatever it was convenient to have connected, nonchalantly implying that everyone knew the general theory. In Robinson's second chapter the problem is given twenty-five pages of careful analysis. Such an analysis meets at once the time-honored issue of the status of associated items when they are not being observed. Few of the scientifically minded subscribe to the doctrine that associated items reside, as objects, in the unconscious during the periods between appearances. To place the seat of permanence of association between objects within the neurograms is no more profitable for, among other things, such a theory "endows the brain with all of the characteristics necessary to explain the observed facts of association" and "throws little light back upon those facts by assuming that their preservation is neurological" (p. 32). What really endures is the probability of certain acts occurring in certain ways, the fact of capacity for such occurrence, "and there should be no objection to talking about such capacities or dispositions as if they were real. They are as real as the potential energy of the vase upon the mantel. They are even as real as the stubborn presence of a carved inscription on a block of granite" (p. 34). Such a view involves neither disembodied functions nor useless phantasy about neural events. Rather it takes psychological dispositions as it finds them and leaves both the problem of neural correlation and of ultimate character for the future.

In describing associative connections, the neutral expressions *instigating* and *instigated* items or acts, as proposed by Hollingworth, are employed in place of the more common stimulus and response. The two latter can be used in dealing with association only by lifting them out of the familiar and legitimate context which has been built up around them. Robinson's critical treatment of the stimulus is a noteworthy feature of the essay. He avoids the current loose usage whereby any antecedent event is a stimulus and any consequent a response. For association, process of stimulation in an organism is one thing and the stimulus which arouses the process is another. The latter is without the realm of associatively connected events.

The question of the items which become associated is, thus, divided into the two questions: What items may serve as instigators and what items may be instigated? As instigators come sensory processes, complete perceptual acts, ideational processes, and affective activities. The inclusion of the last is meant to imply that determining tendencies and general attitudes, as well as analytically isolable details, are associable items. A mood may function as an instigating event and so many numberless other general conditions. In this view, as in the total context of the book, the inveighings of the modern anti-associationists against the allegedly atomistic and perniciously analytic character of associationism are met on positive grounds and shown to be wide of the mark of contemporary theory. Consistently with his general treatment of the stimulus, Robinson holds that muscular movements and postures do not function directly as associative instigators, nor do glandular reactions. These can instigate only through the medium of sensory process.

Given the instigators, what processes are associatively instigatable? Within this class come ideas, feelings, general sets, muscular movements, glandular secretings, and the varieties of inhibition. Sensory processes are excluded. The activities included, while not necessarily irreducible, are held to be units which analysis actually can reach and in terms of which experimental work can proceed.

In the third chapter, on simultaneous and successive association, the orderly sequence of mental events is treated, not as a feature of the stimuli, but as associatively determined. Within this associative order there is held to be overwhelming evidence for the presence of unconscious mediating factors. Simultaneous association, although a difficult and acknowledged abstraction, is a legitimate and useful concept. Not all simultaneous groupings of psychological events, however, are cases of simultaneous association. The author's elaboration of this point and of its sequels contains much sound systematic thinking which reaches beyond the bailiwick of associationism into the universe of general systematic psychology. Insight, the magic wand of gestalt, is construed as equivalent to simultaneous association. "In both cases a complex of simultaneous stimuli produces a state in which the several psychological results of these stimuli enter into a simultaneous organization" (p. 64). The gestaltist proceeds to assume that such an organization, when it precedes a succession of integrated acts, automatically accounts therefor. For this there is no proof. What are the actual principles governing such relations is a matter for experimental examination. It is suggested later on, in the discussion of the laws of association, that this so-called insight phenomenon is covered by the law of assimilation.

These fundamental considerations prepare the way for two chapters on the "laws" of association, to which nearly half of the book is devoted. That these "laws," to Professor Robinson, are not the sacred and final shibboleths which they have been to some may be gleaned from the quotation marks in which he writes the word. That they are tentative formulations, instrumental to experimental and theoretical advance and changing therewith, no one who reads his chapters may forget.

Authors who have distinguished between qualitative and quantitative laws of associative fixation have neglected to make clear exactly what they have meant. Robinson believes that most have had in mind a distinction between more and less fundamental. Upon the proposition that contiguity is a more fundamental law there is general agreement. But is there evidence that in predicting associative connections contiguity should be given more weight than should frequency? None is known and there are no common units in which the two may be compared. Why, moreover, should one desire to guess at relative importance when what is needed is experimental research on all? It is clear that he proposes to consider all of the apparently valid laws as at present coordinate and to waive examination of whether they are qualitative or quantitative in any of the possible meanings of that distinction.

The contiguity of the famous law is temporal. It reads: "*The fact that two psychological processes occur together in time or in immediate succession increases the probability that an associative connection between them will develop—that one process will become the associative instigator of the other*" (p. 72). The subsequent examination of the operation of contiguity not only clarifies the law but casts further doubt upon its unique fundamentality. Mere temporal coincidence or succession will not insure demonstrable association; other factors, such as frequency, may be necessary. The contiguity involved is not, in addition, the all-or-none affair which has often been implied. It is, instead, a continuous variable. On logical grounds "it thus appears that the law of contiguity is a functional relation between two continuous variables—contiguity on the one hand and degree of association on the other. And, of course, if this fits the facts as they are, there is no really good reason for continuing to speak of the law of contiguity as *qualitative*" (p. 74). On the experimental side, such experiments as those of Carr and Yarborough give logic factual assurance. In the reviewer's opinion this treatment of contiguity is of the first importance. It rids the law of ambiguity and clears the ground for research.

The law of assimilation "contradicts the claim that associative connections are between absolutely specific items. Both instigating and instigated activities are always more general than any single observation of them is capable of revealing" (p. 92). Departure from literal stereotypy, orginality in short, "is something which any current theory of association must, itself, predict" (p. 93). Robinson states the law thus: *"Whenever an associative connection is so established that an activity, A, becomes capable of instigating an activity, B, activities other than A also undergo an increase or decrease in their capacity to instigate B"* (p. 86). He adds, also, the extension of the law that *"A will vary in its capacity for instigating certain activities other than B"* (p. 92). The general law may be expected to hold for both instigating and instigated processes. Again associationism is set upon contemporary ground and further bulwarked against its critics.

The laws of frequency, intensity, duration, and context come next. When the data upon curves of learning, upon the work decrement, and upon distribution of practice, among others, are considered, the law of frequency stands out as no all-or-none law but as a law of relative frequency. "We may be sure that the optimal relative frequency for almost any given connection will usually lie between lesser and greater relative frequency values. The principles of refractoriness and fatigue are limiting factors on one side and the principles of forgetting and warming-up are limiting factors on the other side" (pp. 101-102). This view of the frequency law incorporates and organizes the recent experimental attacks upon it.

Variations in several kinds of intensity affect the formation of associations, but how shall a law of intensity be formulated? Does the term refer to sensory intensity, Titchenerian attensity, affective intensity, or to still other kinds? Does it apply to the instigating or to the instigated event, or to both at once? These and analogous questions throw sharply into relief the present unsatisfactory status of the intensity law. That intensity in some form is important is beyond doubt and the need is for experimentation. Of duration the same general considerations may be urged. Its influence is often entwined with that of frequency and investigation should deal with single occurrences of associations varying in absolute length.

The other laws, save only assimilation, abstract for consideration pairs of associated terms, although they do not necessarily forget that an abstraction has been performed. The law of context takes into account the fact that the activities to be associated never occur in isolation and lays again the atomistic ghost. Between the laws of context and of assimilation there is a close relation, but reasons are given for treating them separately. Analyzing in terms of assimilation, one inquires "what identical associative powers can be taken on by a class of activities varying from each other in some specified manner." When one analyzes in terms of context, one asks "what varying associative powers can be taken on by a given activity that occurs in varying context" (p. 113).

To these somewhat more established laws, Professor Robinson adds three others: acquaintance, composition, and individual differences. The very fact of acquaintance with an item prior to its association with others may affect the facility with which it enters into association. Aside from acquaintance and previous association, moreover, there are differences in the readiness with which an activity can be associated with other activities by virtue of the nature of the activities themselves. This is the factor of composition. The third has received more general notice and investigation than have the two first. It is: *"The rate at which any given associative connection can be fixated depends, among other things, upon a factor of idiosyncrasy which cannot be adequately explained in terms of the individual's personal history"* (p. 121).

Whether these factors of which facility of associative formation is a function are "laws" or not is said to offer opportunity for honest doubt. But, to the author, "if the assumption that these factors are important determiners of association be correct, then there are 'laws' of these factors whether our knowledge of them is definite or not. If this point hinges upon a realistic philosophy, it is only the realism by which science lives her everyday life, whatever scientists may say when they make up stories about her" (p. 124).

Beyond the problems discussed lies a host of others, such as those connected with the decay of associative connections and with the relation of association to other principles of psychodynamics. "But the extension of the theory to these other problems involves no important addition to the general viewpoint" (p. 132). In the last chapter, therefore, he turns to a summary of the general characteristics of associationism, a genetic theory. The modern theory lays no claim to ultimacy and none to all-inclusiveness. It is no philosophy of mind. It "is to be regarded simply as an attempt to organize a class of psychological phenomena" (p. 126). It does not imply that all changes in psychological events are associational. Adaptation, refractory phase, perseveration, conflict, are factors also. It is a scientific theory only and, viewed from that frame of reference, many of the critics' shafts are wide of the mark.

The analysis reveals no single associative law, although "throughout the history of associational analysis there has been a recurrent search for the one essential condition of associative connection," for "the great associative *It*" (p. 128). Recently a new manifestation of this tendency has bloomed in the attempt to show that *mere* frequency or *mere* contiguity does not bring about association. There is no single general law save, perhaps, that associative strength is a function of N demonstrable factors.

Professor Robinson's essay is a contribution of the first importance to systematic psychology and to the experimental work of the future. The book is a monument to the urgency of research upon the problems of association. Its crisp statements of where we stand in factual knowledge of these ancestral laws upon which the systematic structure of so much of our psychology rests point forwards; in many cases there is no past of experimental fact to point towards. The emphasis is upon investigation, not on the final and finished principle. To psychologists, educational or otherwise, who have accustomedly taught the "laws of association" as tablets of finality, transmitted from Aristotle through Thorndike and encrusted with the inviolable unction of tradition, the book may be a cold and corrupting wind blowing from the waste places of the unknown to disturb the dogmatic calm of classroom pronouncement. To the experimentalist, even though skeptical of all "laws" by long-established habit, the essay renders skepticism more precise. It restates old criticisms in fresh and illuminating ways, sprouts a dozen new ones where none grew before, and, which is more important, bristles with experimental problems.

A tradition almost as old as contiguity sanctions a reviewer's disagreements with the absent author and even a book as good as this one must come under the sanction. I am puzzled at the absence of mention of effect. Surely, whatever the judgment of its validity, it deserves treatment on the grounds of inherence in the American tradition and of recent experimental liveliness. What, also, of the more recent Thorndikian factors, belongingness, identifiability, and availability? I should have welcomed a somewhat more complete review of the relevant evidence upon the laws, although I grant that it would not have changed the formulation of their status. Welcome, also, would have been a fuller treatment of redintegration, a concept with which, it seems to me, associationism must come to fairer terms than dismissal on the grounds of being but a starting-point for analysis. The objection, however, that more should have been said, whether valid or not, detracts nothing from the high importance of what has been said.

Modern associationism is surely American, but it is not so certainly the custom for American writers on either experiment or system to verbalize in alert and graceful style which avoids the well-known prosaic commonplaces with forceful strokes of its own. The nine laws of association have served their expositor well.

John A. McGeoch

University of Missouri
Columbia, Missouri

R. A. Houstoun. *Vision and Colour Vision.* New York: Longmans, Green, 1932. Pp. vii+238.

At the present time workers in vision are fairly well agreed upon one point: Visual theory, if it is going to be more than a technology of illumination, must be brought into line with other departments of physiology. Contributions made during recent years are witness to this point of view. The photochemical model of Hecht, the work of Adrian and Matthews and Granit on retinal synaptic processes, and the electrical studies of Hartline have forced a recognition of this fundamental attitude.

Houstoun's book, *Vision and Colour Vision,* is an outstanding example of failure to assume this attitude. There is throughout the book a lack of appreciation of the implications forced by the studies on nerve and reflexes, and in some places statements are made which are totally out of accord with known physiological principles. A striking example of this type of error is found in the author's account of acuity. In this treatment he follows Hecht's scheme to some extent, but varies the presentation in order to inject many of his own ideas. On page 74, in speaking of Hecht and Wolf's study on bees, he says: "The ommatidia constitute a homogeneous population with different thresholds. They have no degrees of activity, but are either in action or out of action." Such a statement leads to the inference that Houstoun is unaware of the rôle of frequency. End-organs *do* have degrees of activity,—a fact which could have been inferred from the work of Adrian and Matthews. Recently this reviewer, working with Dr. Hartline, has shown that the range of intensities to which a single photoreceptor unit (eye of *Limulus polyphemus*) responds with varying frequency of nerve impulses may be as great as 1 to 1,000,000. The earlier work of Adrian and Zotterman, Bronk, and Adrian and Bronk inevitably carried the implication that frequency is the primary variant in excitation.

Another criticism which can be levelled agains the author is concerned with his treatment of interaction. In the discussion of luminosity contrast (page 173), he says: "Thus the interaction between the two areas seems to take place in the photochemical layer, before the rods and cones are stimulated, and not higher up, as suggested by McDougall: possibly the electrical condition of the surround alters the potential over the image of the test object." This statement is put forward despite the failure of Adrian and Matthews to explain their interaction effects on any but a synaptic basis, despite the implication of Sherringtonian studies, and despite the fact that retinal luminosity contrast can be described physiologically more satisfactorily as a balance of inhibition and summation than it can be described as a change in the intensity of a stream of electrons.

In line with this disregard of the synaptic possibilities it should be noted that Houstoun avoids as much as possible words which are descriptive of reflex activity—'inhibition,' 'summation,' etc. This is done in spite of the fact that each term has its own exact meaning—a meaning which applies only to a specific kind of neural behavior, and in so applying is descriptive of it. The reviewer is willing to grant that there is nothing intrinsically explanatory in such terms. However, it seems to him that, inasmuch as they are descriptive of certain types of synaptic activity, they (with their implications) constitute a more likely description of certain of the processes in vision than do the more physically satisfactory (but much less probable) hypotheses of changes in intensity of electron stream.

One more criticism can be raised on the grounds of physiological inadequacy. On page 228, in propounding a theory of color vision, Houston says: "We think therefore of a train of pulses of variable frequency approaching the synapse from the cone, the frequency varying according to the color of the incident light." And, further, he says: "Beyond the synapse there are only two possible frequencies, e.g., in the case of the red-green fibres the fibre must be either in the red or green state." In discussing these statements one can say little. They are so out of harmony with physiological doctrine (or at least innuendo) that criticism is beside the point. Any inference that could be made from the facts of nerve behavior would lead one to question the possibility for a fiber to be now in a red state, now in a green state. Further, neural physiology would give little basis for the assumption that frequency depends on wave-length rather than on intensity. Certainly, the reverse would be the expectation.

The book takes up the conventional topics of vision: intensity discrimination, dark adaptation, acuity, recurrent vision and flicker, contrast, color vision, color blindness. The relatively great amount of space devoted to the topics less often encountered in the average treatise (constrast, after-images) is a feature of the work. In the discussion of color vision one misses a presentation of the recent findings of Hecht—analyses which are of major importance. No book on color vision in the future can fail to take account of them, and the value of this publication is lessened because of this lack.

The foregoing discussion makes it evident that the reviewer does not recommend the book. Aside from the bad physiology, there is a vagueness and incoherence in development of subject mater. The author appears to have read a great variety of literature, but he has failed to present it in a concise integrated manner. *Vision and Colour Vision* is unsuitable for the general reader and merits little consideration from the research worker.

Clark University CLARENCE H. GRAHAM
Worcester, Massachusetts

J. W. DUNLAP and A. K. KURTZ. *Handbook of Statistical Nomographs, Tables, and Formulas.* New York: World Book Co., 1932. Pp. vii+163. $6.00.

This handbook, appearing in the *Measurement and Adjustment Series* edited by Terman, offers an invaluable compilation of statistical aids for students and researchers. Twenty-eight nomographs render possible the rapid determination of errors and deviations plus certain other factors when the underlying statistical values are known. Errors in reading are said by the authors not to enter probably before the third or fourth significant figure. Twelve tables of carefully checked numerical data are given, including such material as $1—r^2$ and its square root, reliability of two to ten tests, and functions used in rank correlation. The volume concludes with 434 formulas given with uniform notation. Each formula is accompanied by references to statistical sources where it may be found. The volume closes with a list of symbols, a table of errors found in published formulas, and a very complete index.

Clark University
Worcester, Massachusetts

WALTER S. HUNTER

BOOKS RECEIVED

ALLEN, R. D., BUTTERFIELD, W. H., & TULLY, M. Providence inventory test in music. (Form A.) Yonkers, N. Y.: World Book Co., 1932. Pkg. of 25, $1.10.

BAGLEY, W. C., & MACDONALD, M. E. Standard practices in teaching: a summary of the standards generally recognized as governing good practice in typical teaching procedures. New York: Macmillan, 1932. Pp. viii+189. $2.00.

BERGSON, H. Les deux sources de la morale et de la religion. Paris: Alcan. Pp. 346. 25 fr.

BINGHAM, W. V. [Ed.] Psychology today: lectures and study manual. Chicago: Univ. Chicago Press, 1932. Pp. xii+272+44+48+32+32+32+36. $2.50.

BLACKSTONE, E. G., & MCLAUGHLIN, M. W. Blackstone stenographic proficiency tests: Stenography test. Yonkers, N. Y.: World Book Co., 1932. Pkg. of 25, $1.20.

BRIGHAM, C. C. A study of error. New York: Coll. Entrance Exam. Board, 1932. Pp. xiv+384.

BROWN, A. E. The effectiveness of large classes at the college level: an experimental study involving the size variable and the size-procedure variable. *Univ. Iowa Stud.: Stud. Educ.*, 1932, **7**, No. 3 (New. Ser., No. 232). Pp. 66. $.75.

BUCHANAN, S. Symbolic distance in relation to analogy and fiction. (*Psyche Miniatures, Gen. Ser.*, No. 39.) London: Kegan Paul, 1932. Pp. 110.

CANTRIL, H. General and specific attitudes. *Psychol. Monog.*, 1932, **42**, No. 5 (Whole No. 192). Pp. x+109.

CARRÉ, J.-R. Fontelle: de l'origine des fables (1724). Paris: Alcan 1932. Pp. 103. 20 fr.

DESILVA, H. R. [Trans.] Theoretical psychology, by J. Lindworsky, S. J. St. Louis, Mo., & London: Herder, 1932. Pp. xii+145. $1.25.

DICKINSON, R. L., & BEAM, L. A thousand marriages: a medical study of sex adjustment. (Introduction by H. Ellis.) Baltimore, Md.: Williams & Wilkins, 1931. Pp. xxvi+482. $5.00.

FRÖSCHELS, E., DITTRICH, O., & WILHEIM, I. Psychological elements in speech. (Trans. by N. Ferre.) Boston: Expression Co., 1932. Pp. xviii+270. $5.00.

GAULT, R. H. Criminology. Boston: Heath, 1932. Pp. x+461. $3.48.

GOW, C. R. Elements of human engineering. (Ed. by F. A. Magoun.) New York: Macmillan, 1932. Pp. xiv+170. $1.60.

GRUENDER, H. Experimental psychology. Milwaukee, Wis.: Bruce, 1932. Pp. vii+455. $2.50.

HERSEY, R. B. Workers' emotions in shop and home. Philadelphia: Univ. Pa. Press, 1932. Pp. xviii+441. $3.00.

HOUSTOUN, R. A. Vision and colour vision. London, New York & Toronto: Longmans, Green, 1932. Pp. viii+238. $4.50.

JASTROW, J. The house that Freud built. New York: Greenberg, 1932. Pp. x+293.

KIRKPATRICK, E. A. The sciences of man in the making: an orientation book. New York: Harcourt, Brace; London: Kegan Paul, 1932. Pp. xvi+396. $4.00.

LANDMAN, J. H. Human sterilization. New York: Macmillan, 1932. Pp. xviii+341. $4.00.

MACKINTOSH, H. K. A critical study of children's choices in poetry. *Univ. Iowa Stud.: Stud. Educ.,* 1932, **7**, No. 4 (New Ser., No. 234). Pp. 128. $1.00.

MORRIS, C. W. Six theories of mind. Chicago: Univ. Chicago Press, 1932. Pp. xii+337. $4.00.

MURCHISON, C. [Ed.] A history of psychology in autobiography: Vol. 2, by Bourdon, B., Drever, J., Dunlap, K., Ferrari, G. C., Franz, S. I., Groos, K., Heymans, G., Höffding, H., Judd, C. H., Morgan, C. L., Pillsbury, W. B., Terman, L. M., Washburn, M. F., Woodworth, R. S., Yerkes, R. M. Worcester, Mass.: Clark Univ. Press; London: Oxford Univ. Press, 1932. Pp. xx+407. $5.00.

OSTY, E., & OSTY, M. Les pouvoirs inconnus de l'esprit sur la matière: premières étapes d'une recherche. Paris: Alcan, 1932. Pp. 154. 23 fr.

PETERSON, E. T., LINDQUIST, E. F., JEEP, H. A., & PRICE, M. P. Teacher supply and demand in Iowa. *Univ. Iowa Stud.: Stud. Educ.,* 1932, **7**, No. 2 (New Ser., No. 229). Pp. 464. $2.50.

PETRAN, L. A. An experimental study of pitch recognition. *Psychol. Monog.,* 1932, **42**, No. 6 (Whole No. 193). Pp. 124.

RAIGA, E. L'envie: son rôle social . Paris: Alcan, 1932. Pp. xv+268. 15 fr.

ROBINSON, E. S. Association theory today. New York: Century, 1932. Pp. viii+142. $2.00.

RUCKMICK, C. A., & TRAVIS, L. E. University of Iowa Studies in Psychology, No. 15. *Psychol. Monog.,* 1932, **43**, No. 1 (Whole No. 194). Pp. viii+303.

SEASHORE, C. E. [Ed.] The vibrato. *Univ. Iowa Stud.: Stud. Psychol. Music,* 1932, **1** (New Ser., No. 225). Pp. 382. Paper, $2.00; cloth $2.50.

STONE, C. P., DARROW, C. W., LANDIS, C., & HEATH, L. L. Studies in the dynamics of behavior, ed. by K. S. Lashley. (*Behav. Res. Fund Monog.*) Chicago: Univ. Chicago Press, 1932. Pp. xiv+332. $5.00.

SWAIN, B. Fools and folly during the Middle Ages and the Renaissance. New York: Columbia Univ. Press, 1932. Pp. viii+234. $3.00.

THORNDIKE, E. L. The fundamentals of learning. New York: Bur. Publ., Teach. Coll., Columbia Univ., 1932. Pp. xviii+638.

TROLAND, L. T. The principles of psychophysiology; a survey of modern scientific psychology: Vol. 3. Cerebration and action. New York: Van Nostrand, 1932. Pp. xxiv+446.

WARDEN, C. J. The evolution of human behavior. New York: Macmillan, 1932. Pp. xii+248. $3.00.

Wheeler, R. H., & Perkins, F. T. Principles of mental development: a textbook in educational psychology. New York: Crowell, 1932. Pp. xxviii+529. $3.75.

Zafiropulo, J. La philosophie affective: essai d'une métaphysique absolue. Paris: Alcan, 1932. Pp. 136. 18 fr.

Zuckerman, S. The social life of monkeys and apes. New York: Harcourt, Brace, 1932. Pp. xii+356. $3.75.